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ON THE ORBIT OF DENNING'S COMET (1881 V.)

By WILLIAM E. PLUMMER.

Of the several periodic comets with which we are acquainted, that whose orbit is discussed in the following pages, and for whose discovery we are indebted to Mr. Denning, will probably prove to be not the least interesting, if continued observation at successive returns be practicable. Accepting, for the present, the elements of Dr. Hartwig, published in the Astronomische Nachrichten No. 2405, which are very approximate, and on which the calculations subsequently referred to in this paper have been founded, we have the following data—

$$\begin{array}{rl} T = & 1881,\ \text{Sept. } 13{\cdot}4263 \text{ Berlin Mean Time.} \\ \pi - \Omega = & 312^\circ\ \ 39'\ \ 36''{\cdot}2 \\ \Omega = & \left.\begin{array}{l} 65\ \ \ 54\ \ \ 14{\cdot}8 \\ \ \ 6\ \ \ 50\ \ \ 43{\cdot}4 \end{array}\right\} \text{M. Eq. } 1881{\cdot}0. \\ i = & \\ \phi = & 56\ \ \ \ 7\ \ \ \ 4{\cdot}4 \\ \log a = & 0{\cdot}630750 \\ \text{Period} = & 8{\cdot}8335 \text{ years.} \end{array}$$

These elements present some striking peculiarities, for in the case of the comets of short period hitherto observed, the aphelia have grouped themselves at a distance from the Sun but slightly removed from the orbit of Jupiter, or as in the case of Tuttle's Comet near the orbit of Saturn, but this division hitherto marked with tolerable distinctness, is to some extent removed by the discovery of a comet whose mean distance corresponds to a period of nearly nine years, and which considerably bridges the gap hitherto noticed in the aphelion distances. The period exceeds by more than a year that of Faye's Comet, the most distant of the short period group (properly so called) and falls about midway between that of Encke and Tuttle, indicating a mean motion differing considerably from that of any comet yet observed. The inclination of the orbit to the ecliptic is so small that the comet's distance from that plane is never very great, and consequently its approach to each of the planets can be within very narrow limits. Dr. Hartwig has called attention to this fact and has given the positions and distances of the comet from the principal planets at the time of closest approach as follows:—

Hel. long. of Comet,		32°	Distance from		Venus	0·0215
„	„	83·5	„	„	Earth	0·0359
„	„	174	„	„	Jupiter	0·50
„	„	222	„	„	Jupiter	0·164

If therefore the comet has been a member of our system for any considerable time (and considering its faintness at the last apparition this is not improbable) the orbit must have suffered considerable perturbation from any or all of these planets : a fact, which while it may account for the unusual mean distance that the comet now has, renders any attempt to inquire into its past history, with precision, a matter of great difficulty and uncertainty. The following discussion is therefore confined entirely to the observations secured at its last appearance.

It is also curious to remark that the longitude of the ascending node of this comet corresponds almost exactly with that of the descending node of Biela's Comet. This latter longitude is given by Dr. Hubbard (for the mean Equinox of 1852) as 65° 51′ 28″, in which position the radius vector is 0·9745. The radius vector of Denning's Comet at its ascending node is 0·8500. Of course no great importance can be attached to this similarity of longitude, for if the number of known comets be increased sufficiently, some such coincidences must of necessity be observed. It adds however another point of interest to this particular comet and makes us regret that at this, the first apparition at which the comet has been certainly observed, the number of observations is so limited.

The total number of observations that I have been able to collect is under forty, and the whole of these are not available for rigorous discussion, owing to the introduction of evident but possibly unavoidable errors in the determination of the comet's place. Dr. Winnecke called attention in the Monthly Notices of the Royal Astronomical Society for Nov. 1881 to the desirability of continued observation of this comet by the powerful telescopes in use in this country, but coming as his warning did at a time of year when our climate is proverbially unfitted for the observation of any, and especially of faint objects, his caution, so far as I have been able to learn, was unproductive of any effect. Before entering into the details of the calculations I wish particularly to acknowledge my indebtedness to those directors of foreign observatories who have anticipated the publication of their observations by forwarding me positions of the comet, and I may here mention that I am particularly indebted to Dr. L. Weinek, of Leipzig, who, as will be seen in the sequel, determined anew the coordinates of those comparison stars, whose places had been taken from catalogues of so remote a date that the deduced positions had not the desired accuracy.

Of the several sets of elliptic elements that were published during the time of the comet's appearance the most accurate is apparently the second set of elements deduced by Dr. Hartwig, and referred to in the beginning of this paper. This fact is shown by the comparison which that astronomer has made between some of the observations and the places calculated from the elements, with the following result—

	$d\lambda$ (C — O)	$d\beta$ (C — O)
Oct. 5	+ 1″·6	+ 0″·2
12	— 8·2	— 8·9
19	— 2·1	— 4·1
28	+ 1·0	+ 0·1
Nov. 19	+ 3·2	— 0·5

I have not been able to reproduce these figures exactly, possibly from the employment of different solar tables, but the agreement is sufficiently close to show that these elements can receive but slight alteration, and suffice amply for the purpose of forming normal places, of computing the perturbations, and for the formation of the Equations of condition. To effect the first of these purposes the following Ephemeris has been computed. The Solar Longitudes employed are those taken directly from the Nautical Almanac, as at the time of the computation I was not in possession of any correction to the Solar Tables; a correction which the kindness of the Astronomer Royal subsequently placed at my disposal and which will be referred to later on. For the present, too, the Latitude of the Sun has been neglected.

Ephemeris of Denning's Comet for Greenwich Mean Midnight.

Date, 1881.	Apparent Right Ascension.	Apparent Declination, North.	Log. Distance of Comet from Earth.	Time for Aberration.
October 4,	140° 40′ 48″·1	14° 38′ 15″·5	9·8844038	6^{m} 21^{s}·45
5,	141 15 16·2	14 40 56·7		
6,	141 49 33·6	14 43 14·2		
7,	142 23 39·0	14 45 9·6		
8,	142 57 30·8	14 46 44·4	9·9081470	6 42·89
9,	143 31 7·5	14 48 0·3		
10,	144 4 27·7	14 48 58·8		
11,	144 37 29·8	14 49 41·6		
12,	145 10 13·0	14 50 9·9	9·9289536	7 2·66
13,	145 42 35·8	14 50 25·1		
14,	146 14 37·1	14 50 28·6		
15,	146 46 15·8	14 50 21·7		
16,	147 17 30·9	14 50 5·5	9·9470961	7 20·69
17,	147 48 21·6	14 49 41·3		
18,	148 18 47·1	14 49 10·2		
19,	148 48 46·5	14 48 33·3		
20,	149 18 19·2	14 47 51·4	9·9628354	7 36·95
21,	149 47 24·6	14 47 5·6		
22,	150 16 2·1	14 46 16·8		
23,	150 44 11·1	14 45 26·0		
24,	151 11 51·2	14 44 33·9	9·9764097	7 51·46
25,	151 39 1·9	14 43 41·5		
26,	152 5 42·7	14 42 49·4		
27,	152 31 53·4	14 41 58·4		
28,	152 57 33·6	14 41 9·2	9·9880368	8 4·25

Date, 1881.	Apparent Right Ascension.	Apparent Declination, North.	Log. Distance of Comet from Earth.	Time for Aberration.
October 29,	153° 22′ 43″·1	14° 40′ 22″·5		
30,	153 47 21·5	14 39 38·9		
31,	154 11 28·6	14 38 59·0		
November 1,	154 35 4·3	14 38 23·4	9·9979171	8ᵐ 15ˢ·40
2,	154 58 8·4	14 37 52·6		
3,	155 20 40·7	14 37 27·1		
4,	155 42 40·9	14 37 7·5		
5,	156 4 8·9	14 36 54·1	0·0062209	8 24·96
6,	156 25 4·5	14 36 47·7		
7,	156 45 27·4	14 36 48·7		
8,	157 5 17·5	14 36 57·5		
9,	157 24 34·5	14 37 14·5	0·0130768	8 33·00
10,	157 43 18·0	14 37 40·3		
11,	158 1 27·7	14 38 15·3		
12,	158 19 3·4	14 38 59·9		
13,	158 36 4·7	14 39 54·5	0·0186812	8 39·66
14,	158 52 31·3	14 40 59·6		
15,	159 8 23·0	14 42 15·6		
16,	159 23 39·3	14 43 42·8		
17,	159 38 20·0	14 45 21·7	0·0231090	8 44·98
18,	159 52 24·8	14 47 12·4		
19,	160 5 53·3	14 49 15·3		
20,	160 18 45·3	14 51 30·8		
21,	160 31 0·4	14 53 59·2	0·0265134	8 49·11

The Geocentric motion in Declination is curious. During the whole seven weeks the comet remained visible the motion is confined to some fifteen minutes of North Polar Distance.

On the completion of the Ephemeris a preliminary comparison with the observations made at the more important observatories was instituted, for the purpose of detecting, if possible, any pecularities in the mode of observing, with a view to attaching additional weight to the apparently more trustworthy results. The number of observations was however too scanty to permit any decisive inferences, and consequently uniform weight (irrespective of the number of comparisons, a detail which the observers have not given in all cases) has been given to all observations. The results, which possess some interest, are given below. It would appear that at Strassburg the point in the comet selected for observation lies considerably to the east of that chosen by other observers, but the observations are not sufficiently numerous to give decisive information on this question.

Date.	(C – O) Δα	(C – O) Δδ	Date	(C – O) Δα	(C – O) Δδ
	Dun Echt.			**Paris.**	
	″	″		″	″
Oct. 9·7	+ 12·65	+ 4·48	Oct. 17·6	+ 9·93	– 0·02
10·7	+ 19·63	+ 4·41	18·6	+ 12·76	+ 4·17
12·6	+ 11·09	+ 4·90	30·6	+ 13·41	– 2·97
14·7	+ 11·75	+ 0·91			
				Rome.	
			Oct. 17·7	+ 11·22	+ 7·36
	Harvard College.		18·6	+ 23·20	– 0·85
			21·6	+ 10·76	+ 6·88
Oct. 10·9	+ 22·06	– 3·31	26·6	+ 6·05	+ 20·37
10·9	+ 15·90	+ 5·85	Nov. 3·7	. . . .	+ 7·34
11·8	+ 6·33	– 3·90			
18·9	+ 19·62	+ 3·32			
20·9	+ 20·44	+ 2·24		**Strassburg.**	
21·9	+ 18·38	+ 2·94	Oct. 18·7	+ 4·47	+ 1·47
25·9	+ 32·34	+ 7·24	19·6	+ 4·47	+ 2·93
26·9	+ 4·48	+ 1·30	28·6	+ 1·31	– 0·82
27·9	+ 17·34	– 5·37	Nov. 19·7	– 2·80	+ 2·48

The perturbations of the comet during the time it was visible were next computed, for four day intervals by the method of variation of constants. The influence of each of the planets from Mercury to Saturn inclusive was taken into account, and the effect on each of the elements is shown in the following table. Owing to the formula employed in integrating, the elements are made to osculate from 1881 Sept. 28, Greenwich midnight, at which date the Mean Anomaly given by Dr. Hartwig's elements is 1° 41′ 9″·76.

Date.	$d\pi$	$d\Omega$	di	$d\phi$	dM	$d\mu$
Oct. 4·5	– 0″·371	– 0″·025	+ 0″·016	– 0″·398	– 0″·317	+ 0″·00378
8·5	– 0·658	– 0·029	+ 0·024	– 0·677	– 0·548	·00683
12·5	– 0·998	– 0·025	+ 0·032	– 1·073	– 0·813	·01097
16·5	– 1·396	– 0·014	+ 0·040	– 1·571	– 1·117	·01618
20·5	– 1·860	+ 0·002	+ 0·048	– 2·160	– 1·461	·02235
24·5	– 2·379	+ 0·023	+ 0·055	– 2·816	– 1·840	+ 0·02926
28·5	– 2·938	+ 0·049	+ 0·061	– 3·523	– 2·249	0·3672
Nov. 1·5	– 3·526	+ 0·076	+ 0·067	– 4·267	– 2·684	·04459
5·5	– 4·136	+ 0·104	+ 0·073	– 5·044	– 3·138	·05281
9·5	– 4·777	+ 0·132	+ 0·078	– 5·859	– 3·610	·06143
13·5	– 5·452	+ 0·162	+ 0·082	– 6·708	– 4·107	+ 0·07046
17·5	– 6·168	+ 0·193	+ 0·086	– 7·583	– 4·636	·07981
21·5	– 6·916	+ 0·225	+ 0·090	– 8·462	– 5·194	·08928

The effect of these perturbations upon the observed Right Ascension and Declination of the comet will be most readily and conveniently determined by computing the differential coefficients for the same epoch as those in the Table.

As these differential coefficients were also used in the formation of the equations of condition either directly or by interpolation it will be desirable and sufficient to give them here. For an additional check upon the accuracy of the work, beyond the ordinary method of differencing, in one case, Nov. 21·5, the small variations were applied directly to the elements and the spherical coordinates of the comet computed from the elements of the instantaneous ellipse agreed exactly with those derived from the employment of the appropriate factors.

Oct.	4·5	$\cos\delta\Delta\alpha$	+ 0·2519 $d\pi$	– 0·0362 $d\Omega$	– 0·0278 di	+ 2·9504 $d\phi$	– 1·3643 dM	+ 333·97 $d\mu$
	8·5		+ 0·3201	– 0·0383	+ 0·0052	+ 2·8735	– 0·8902	+ 320·21
	12·5		+ 0·3800	– 0·0400	+ 0·0380	+ 2·8222	– 0·5410	+ 311·05
	16·5		+ 0·4333	– 0·0415	+ 0·0705	+ 2·7870	– 0·2872	+ 305·17
	20·5		+ 0·4823	– 0·0426	+ 0·1025	+ 2·7654	– 0·0970	+ 301·79
	24·5		+ 0·5279	– 0·0437	+ 0·1341	+ 2·7530	+ 0·0423	+ 300·25
	28·5		+ 0·5716	– 0·0445	+ 0·1653	+ 2·7492	+ 0·1499	+ 300·20
Nov.	1·5		+ 0·6143	– 0·0452	+ 0·1959	+ 2·7541	+ 0·2381	+ 301·40
	5·5		+ 0·6565	– 0·0458	+ 0·2260	+ 2·7660	+ 0·3121	+ 303·73
	9·5		+ 0·6987	– 0·0463	+ 0·2556	+ 2·7850	+ 0·3767	+ 307·02
	13·5		+ 0·7419	– 0·0467	+ 0·2847	+ 2·8128	+ 0·4425	+ 311·25
	17·5		+ 0·7864	– 0·0470	+ 0·3134	+ 2·8485	+ 0·5093	+ 316·41
	21·5		+ 0·8324	– 0·0472	+ 0·3416	+ 2·8923	+ 0·5795	+ 322·45
Oct.	4·5	$\Delta\delta$	+ 0·0654 $d\pi$	– 0·1218 $d\Omega$	– 0·0976 di	– 0·4728 $d\phi$	+ 2·6121 dM	– 91·69
	8·5		+ 0·0233	– 0·1199	+ 0·0175	– 0·5204	+ 2·1225	– 89·70
	12·5		– 0·0152	– 0·1182	+ 0·1207	– 0·5711	+ 1·7244	– 89·51
	16·5		– 0·0509	– 0·1161	+ 0·2142	– 0·6258	+ 1·3973	– 90·85
	20·5		– 0·0847	– 0·1139	+ 0·2997	– 0·6819	+ 1·1239	– 93·22
	24·5		– 0·1170	– 0·1118	+ 0·3788	– 0·7387	+ 0·8939	– 96·37
	28·5		– 0·1480	– 0·1097	+ 0·4526	– 0·7946	+ 0·7004	– 100·05
Nov.	1·5		– 0·1783	– 0·1077	+ 0·5224	– 0·8507	+ 0·5308	– 104·20
	5·5		– 0·2084	– 0·1057	+ 0·5889	– 0·9077	+ 0·3781	– 108·70
	9·5		– 0·2381	– 0·1038	+ 0·6529	– 0·9643	+ 0·2424	– 113·49
	13·5		– 0·2681	– 0·1020	+ 0·7150	– 1·0222	+ 0·1143	– 118·57
	17·5		– 0·2980	– 0·1003	+ 0·7758	– 1·0802	– 0·0042	– 123·87
	21·5		– 0·3286	– 0·0987	+ 0·8356	– 1·1405	– 0·1209	– 129·41

The combination of this last table with that immediately preceding gives the perturbative effect in Right Ascension and Declination. The result is as follows :—

Date.	$\Delta\alpha\cos\delta$	$\Delta\delta$	Date.	$\Delta\alpha\cos\delta$	$d\Delta$
	″	″		″	″
Sept. 28·5	0·00	0·00	Oct. 28·5	– 0·67	– 2·00
Oct. 4·5	+ 0·43	– 1·01	Nov. 1·5	– 1·10	– 1·78
8·5	+ 0·48	– 1·43	5·5	– 1·58	– 1·46
12·5	+ 0·44	– 1·78	9·5	– 2·14	– 1·02
16·5	+ 0·28	– 1·97	13·5	– 2·80	– 0·46
20·5	+ 0·03	– 2·08	17·5	– 3·54	+ 0·21
24·5	– 0·30	– 2·08	21·5	– 4·42	+ 1·06

Before, however, the normal places can be formed and the deviations from the path from the osculating ellipse can be determined it is important to introduce two corrections ; the one depending upon the improved places of the stars of comparison and the other on the employment of corrected Solar Longitudes in the calculation of the Ephemeris. For the first of these two corrections I am indebted to Dr. Weinek, of Leipzig, in whose charge is that zone in which are to be met all the stars employed. The position of the stars given in the following table are generally the mean of two determinations expressly observed by that Astronomer for the purposes of this inquiry. The Equinox to which the observations are referred is 1881·0—

Adopted R.A. of Comparison Star.	Corrected Seconds of R.A. supplied by Dr. Weinek.	Correction to observed R.A. of Comet.	Adopted Declination of Comparison Star.	Corrected Seconds of Declination supplied by Dr. Weinek.	Correction to Observed Declination of Comet.	Place of Observation.
9^{h} 37^{m} 14^{s}·78	15^{s}·00	+ 3″·30	+ 14° 33′ 55″·90	55″·31	− 0″·59	Harv. College.
9 37 17·96	18·24	+ 4·20	+ 15 4 44·20	46·15	+ 1·95	Harv. College.
9 53 33·38	33·30	− 1·20	+ 14 49 55·40	55·68	+ 0·28	Rome.
9 53 33·06	33·30	+ 3·60	+ 14 49 56·10	55·68	− 0·42	Paris.
9 55 46·48	46·23	− 3·75	+ 14 51 29·30	29·64	+ 0·34	Strassburg.
10 0 37·16	36·79	− 5·55	+ 14 44 20·60	14·84	− 5·76	Palermo.
10 5 27·92	28·57	+ 9·75	+ 14 39 54·40	67·80	+ 13·40	Odessa.
10 7 32·76	32·82	+ 0·90	+ 14 27 37·80	39·29	+ 1·49	Odessa.
10 8 8·27	8·34	+ 1·05	+ 14 53 50·30	46·46	− 3·84	Odessa.
10 10 17·59	17·44	− 2·25	+ 14 19 14·70	15·91	+ 1·21	Palermo.
10 10 17·44	17·44	0·00	+ 14 19 18·22	15·91	− 2·31	Athens.
10 12 2·99	2·60	− 5·85	+ 14 31 41·50	41·67	+ 0·17	Rome.
10 12 2·96	2·60	− 5·40	+ 14 31 41·30	41·67	+ 0·37	Harv. College.
10 21 29·43	29·15	− 4·20	+ 14 52 40·99	35·67	− 5·32	Athens.
10 23 26·35	26·21	− 2·10	+ 14 45 11·20	12·06	+ 0·86	Rome.
10 23 26·16	26·21	+ 0·75	+ 14 45 12·60	12·06	− 0·54	Marseilles.

In the case of an observation made at Marseilles on October 5 the place of the comparison star (7 Leonis) has been taken from the British Association Catalogue This star has recently been observed at Greenwich and it appears that the position in the earlier catalogue requires a correction in R.A. of − 0″·90 and in North Polar Distance of − 4″·01.

The data for the correction of the Solar Longitudes, given in the Nautical Almanac, was furnished by the Astronomer Royal and is given in the following Table—

Apparent Errors of the Solar Tables in R.A. and N.P.D. from October 13 to November 19, 1881, from Observations made at the Royal Observatory, Greenwich.

Day and Hour of Observation.	Apparent Error of Tables in R.A.	Apparent Error of Tables in N.P.D.	Deduced Error in Longitude.	Day and Hour of Observation.	Apparent Error of Tables in R.A.	Apparent Error of Tables in N.P.D.	Deduced Error in Longitude.
d. h.	s.	″	″	d. h.	s.	″	″
Oct. 13 0	− 0·15	. . .	. . .	Oct. 31 0	+ 0·07	+ 0·46	+ 1·11
17 0	+ 0·08	+ 0·18	+ 1·17	Nov. 5 0	+ 0·04	+ 2·27	+ 1·24
18 0	+ 0·07	+ 1·38	+ 1·47	9 0	+ 0·06	− 1·08	+ 0·52
19 0	+ 0·09	+ 0·63	+ 1·47	15 0	+ 0·07	− 1·25	+ 0·41
20 0	+ 0·08	+ 0·65	+ 1·33	17 0	+ 0·07	+ 0·27	+ 1·03
25 0	. . .	+ 0·37	. . .	18 0	0·00	. . .	. .
26 0	+ 0·07	+ 0·54	+ 1·14	19 0	− 0·03	+ 1·06	− 0·17
28 0	+ 0·20	+ 0·07	+ 2·77				

As, unfortunately, no observation of the Sun was made at Greenwich between Oct. 4 and Oct. 13, I have not attempted to make any correction to the earlier positions of the Ephemeris. For the latter portion a formula of the form $A + Bt + Ct^2 = \Delta\theta$ is best satisfied by the equation—

$$\Delta\theta = + 1''{\cdot}70 - 0''{\cdot}021\, t - 0''{\cdot}0054\, t^2$$

and the small corrections, computed from this formula and exhibited in the adjoining table, are those which have been actually used in the correction of the Ephemeris.

The corrections applied to the Solar Longitudes given in the Nautical Almanac are—

October 12·5	− 0″·04	November 5·5	+ 1″·53
16·5	+ 0·64	9·5	+ 1·19
20·5	1·17	13·5	+ 0·67
24·5	+ 1·52	17·5	0·00
28·5	+ 1·70	19·5	− 0·89
November 1·5	+ 1·70		

where the sign (+) implies that the tabular longitude is too great. The small correction depending on the Latitude of the Sun has been computed from the usual formula. As a matter of convenience both this and the last-mentioned correction have been applied with the sign changed to the observations.

The correction for parallax needs no special mention further than that the Horizontal Equatoreal Parallax of the Sun has been assumed 8″·848. All the factors of parallax have been recomputed to ensure uniformity.

TABLE exhibiting the Observations, in Right Ascension, fully corrected, with the deviations from the Ephemeris.

Place and Date of Observation, G.M.T.	R.A. given by the Observer.	Correction for Parallax.	Correction to Star's R.A.	Correction for Snn's Long.	Correction for Sun's Lat.	Perturbation.	Adopted Seconds of Comet's R.A.	Tabular R.A.	Error of the Tabular Place.
ATHENS.									
Oct. 27·642442	152° 35′ 7″·05	− 4″·69	− 0″·00	+ 0″·82	− 0″·18	+ 0″·60	3″·60	34″·65	+ 31″·05
Nov. 3·632940	155 24 28·35	− 4·43	− 4·20	+ 0·61	− 0·24	+ 1·40	21·49	38·06	− 43·43
DUN ECHT.									
Oct. 9·700018	143 37 33·75	− 4·86	. .	. .	− 0·12	− 0·49	28·28	48·89	+ 20·61
10·669595	144 9 50·85	− 5·37	. .	. .	− 0·07	− 0·48	44·93	65·11	+ 20·18
12·625584	145 14 12·75	− 5·74	. .	− 0·04	+ 0·03	− 0·45	6·55	18·10	+ 11·55
14·703385	146 20 57·90	− 4·38	. .	+ 0·23	+ 0·09	− 0·39	53·45	65·09	+ 11·64

TABLE exhibiting the Observations, in Right Ascension—*continued.*

Place and Date of Observation, G.M.T.	R.A. given by the Observer.	Correction for Parallax.	Correction to Star's R.A.	Correction for Sun's Long.	Correction for Sun's Lat.	Perturbation.	Adopted Seconds of Comet's R.A.	Tabular R.A.	Error of the Tabular Place.
HARVARD COLLEGE.									
Oct. 10·863270	144° 16′ 15″·00	– 7″·23	+ 3″·30	. .	– 0″·06	– 0″·47	10″·54	29″·83	+ 19″·29
10·871302	144 16 26·90	– 7·04	+ 4·20	. .	– 0·06	– 0·47	23·53	45·76	+ 22·23
11·837916	144 48 36·60	– 7·62	. .	. .	– 0·02	– 0·46	28·50	35·31	+ 6·81
18·899368	148 30 34·65	– 5·36	. .	+ 0″·53	+ 0·13	– 0·16	29·79	48·85	+ 19·06
20·885544	149 29 20·55	– 5·63	. .	+ 0·62	+ 0·10	– 0·01	15·63	35·36	+ 19·73
21·857071	149 57 28·95	– 6·30	. .	+ 0·66	+ 0·07	+ 0·06	23·44	41·06	+ 17·62
25·865393	151 48 23·70	– 5·76	. .	+ 0·81	– 0·09	+ 0·41	19·07	50·28	+ 31·21
26·904537	152 16 21·75	– 4·50	– 5·40	+ 0·83	– 0·14	+ 0·51	13·05	21·73	+ 8·68
27·884375	152 41 36·75	– 5·07	– 5·40	+ 0·81	– 0·19	+ 0·60	27·50	49·02	+ 21·52
MARSEILLES.									
Oct. 5·69212	141 21 57·30	– 6·55	– 0·90	. .	– 0·25	– 0·46	49·14	53·72	+ 4·58
Nov. 3·692905	155 24 56·55	– 3·79	+ 0·75	+ 0·61	– 0·24	+ 1·40	55·28	57·87	+ 2·59
ODESSA.									
Oct. 25·571783	151 40 50·85	– 5·64	+ 9·75	+ 0·81	– 0·10	+ 0·40	56·07	57·80	+ 1·73
25·574248	151 40 55·20	– 5·58	+ 0·90	+ 0·81	– 0·10	+ 0·40	51·63	61·78	+ 10·15
26·577238	152 6 10·20	– 5·44	+ 1·05	+ 0·83	– 0·13	+ 0·50	7·01	105·08	+ 98·07
OXFORD.									
Oct. 21·645467	149 51 24·16	– 5·61	. .	+ 0·65	+ 0·05	+ 0·05	19·30	35·60	+ 16·30
28·646868	153 1 9·18	– 5·14	. .	+ 0·80	– 0·22	+ 0·70	5·32	17·22	+ 11·90
PALERMO.									
Oct. 22·619907	150 19 26·85	– 6·69	– 5·55	+ 0·71	+ 0·05	+ 0·13	15·50	26·12	+ 10·62
26·628090	152 9 18·45	– 6·14	. .	+ 0·83	– 0·13	+ 0·50	13·51	5·57	– 7·94
PARIS.									
Oct. 17·609432	147 51 37·65	– 6·60	. .	+ 0·46	+ 0·13	– 0·23	31·41	42·60	+ 11·19
18·605840	148 21 51·00	– 6·55	+ 3·60	+ 0·52	+ 0·14	– 0·17	48·54	58·79	+ 10·25
30·646423	153 50 46·05	– 5·12	. .	+ 0·75	– 0·25	+ 0·92	42·35	55·34	+ 12·99
ROME.									
Oct. 17·674954	147 53 36.75	– 5·28	– 1·20	+ 0·46	+ 0·13	– 0·23	30·63	42·79	+ 12·16
18·615065	148 21 59·10	– 6·84	– 1·20	+ 0·51	+ 0·14	– 0·17	51·54	75·46	+ 23·92
21·618749	149 50 45·75	– 6·50	– 5·55	+ 0·65	+ 0·05	+ 0·05	34·45	50·01	+ 15·56
26·601759	152 8 24·30	– 6·45	– 5·85	+ 0·83	– 0·13	+ 0·50	13·20	23·90	+ 10·70
Nov. 3·683542	155 24 66·75	– 3·51	– 2·10	+ 0·61	– 0·24	+ 1·40	62·91	45·42	– 17·49
STRASSBURG.									
Oct. 18·684335	148 24 21·00	– 4·72	. .	+ 0·51	+ 0·14	– 0·17	16·76	20·75	+ 3·99
19·665942	148 53 43·20	– 5·16	– 3·75	+ 0·56	+ 0·13	– 0·10	34·88	42·51	+ 7·63
28·595862	153 0 4·20	– 5·89	. .	+ 0·80	– 0·22	+ 0·70	59·59	59·62	+ 0·03
Nov. 19·671401	160 8 13·80	– 2·79	. .	– 0·06	+ 0·15	+ 3·97	15·07	8·21	– 6·86

OBSERVATIONS OF DECLINATION.

Place and Date of Observation, G.M.T.	Declination of Comet given by the Observer.	Correction for Parallax.	Correction to Star's Declination.	Correction for Sun's Long.	Correction for Sun's Lat.	Perturbations.	Adopted Seconds of Comet's Declination.	Tabular Declination.	Error of Tabular Declination.
ATHENS.									
Oct. 27·642442	+ 14° 42′ 12″·80	+3″·99	—2″·31	—0″·20	—0″·45	+2″·02	15″·85	51″·27	—24″·58
Nov. 3·632940	+ 14 36 56·40	+ 3·83	— 5·32	— 0·13	— 0·66	+ 1·61	55·73	84·15	+ 28·42
DUN ECHT.									
Oct. 9·750018	+ 14 38 1·40	+ 7·81	. .	. .	— 0·37	+ 1·53	10·37	13·39	+ 3·02
10·669595	+ 14 48 54·80	+ 7·95	. .	. .	— 0·24	+ 1·62	64·13	67·16	+ 3·03
12·625584	+ 14 49 59·50	+ 8·13	. .	— 0·01	+ 0·04	+ 1·78	69·44	72·53	+ 3·09
14·686933	+ 14 50 19·80	+ 7·44	. .	— 0·08	+ 0·27	+ 1·89	29·32	28·10	— 1·22
HARV. COLLEGE.									
Oct. 10·863270	+ 14 49 13·50	+ 5·97	— 0·59	. .	— 0·20	+ 1·64	20·32	16·16	— 4·16
10·871302	+ 14 49 4·80	+ 5·88	+ 1·95	. .	— 0·20	+ 1·64	14·07	16·53	+ 2·46
11·837916	+ 14 49 50·50	+ 6·18	. .	. .	— 0·08	+ 1·72	58·32	52·78	— 5·54
18·899368	+ 14 48 47·80	+ 5·04	. .	— 0·15	+ 0·40	+ 2·03	55·12	56·16	+ 1·04
20·885544	+ 14 47 26·90	+ 5·06	. .	— 0·18	+ 0·32	+ 2·08	34·18	34·20	+ 0·02
21·857071	+ 14 46 40·30	+ 5·28	. .	— 0·20	+ 0·24	+ 2·09	47·71	48·52	+ 0·81
25·865393	+ 14 43 10·30	+ 4·99	. .	— 0·21	— 0·23	+ 2·06	16·91	22·44	+ 5·53
26·904537	+ 14 42 22·70	+ 4·64	+ 0·37	— 0·21	— 0·38	+ 2·04	29·16	28·64	— 0·52
27·884375	+ 14 41 39·90	+ 4·74	+ 0·37	— 0·20	— 0·48	+ 2·01	46·34	39·27	— 7·07
MARSEILLES.									
Oct. 5·69212	+ 14 41 16·50	+ 6·14	+ 4·01	. .	— 0·78	+ 1·13	27·00	25·05	— 1·95
Nov. 3·692905	+ 14 37 15·00	+ 4·48	— 0·54	— 0·13	— 0·62	+ 1·61	19·80	22·86	+ 3·06
ODESSA.									
Oct. 25·571783	+ 14 43 15·00	+ 5·63	+ 13·40	— 0·21	— 0·21	+ 2·06	35·67	37·75	+ 2·08
25·574248	+ 14 43 15·40	+ 5·61	+ 1·49	— 0·21	— 0·21	+ 2·06	24·14	37·62	+ 13·48
26·577238	+ 14 42 31·10	+ 5·53	— 3·84	— 0·21	— 0·37	+ 2·05	34·26	45·53	+ 21·27
OXFORD.									
Oct. 21·645467	+ 14 46 52·06	+ 6·62	. .	— 0·20	+ 0·26	+ 2·09	60·83	58·69	— 2·14
28·646868	+ 14 41 50·40	+ 6·21	. .	— 0·19	— 0·59	+ 2·00	57·83	62·19	+ 4·36
PALERMO.									
Oct. 22·619907	+ 14 45 47·60	+ 4·67	— 5·76	— 0·21	+ 0·17	+ 2·10	48·57	70·82	+ 22·25
26·628090	+ 14 43 7·42	+ 4·41	. .	— 0·21	— 0·37	+ 2·05	13·30	42·81	— 30·49
PARIS.									
Oct. 17·609432	+ 14 49 32·20	+ 6·81	. .	— 0·14	+ 0·39	+ 2·00	41·26	38·23	— 3·03
18·605840	+ 14 48 56·40	+ 6·77	— 0·42	— 0·15	+ 0·40	+ 2·03	5·03	6·57	+ 1·54
30·646423	+ 14 39 30·10	+ 5·70	. .	— 0·17	— 0·70	+ 1·91	36·84	32·83	— 4·01
ROME.									
Oct. 17·674954	+ 14 49 24·00	+ 4·99	+ 0·28	— 0·14	+ 0·39	+ 2·00	31·52	36·35	+ 4·83
18·615065	+ 14 49 1·60	+ 5·50	+ 0·28	— 0·15	+ 0·40	+ 2·03	9·66	6·25	— 3·41
21·618749	+ 14 46 47·80	+ 5·28	— 5·76	— 0·20	+ 0·26	+ 2·09	49·47	59·96	+ 10·49
26·601759	+ 14 42 18·60	+ 5·19	+ 0·17	— 0·21	— 0·37	+ 2·05	25·43	44·16	+ 18·73
Nov. 3·683542	+ 14 36 11·50	+ 4·22	+ 0·86	— 0·13	— 0·65	+ 1·61	17·41	23·06	+ 5·65
STRASSBURG.									
Oct. 18·684335	+ 14 48 56·30	+ 5·56	. .	— 0·15	+ 0·40	+ 2·03	4·14	3·83	— 0·31
19·665942	+ 14 48 18·20	+ 5·68	+ 0·34	— 0·17	+ 0·37	+ 2·06	26·48	26·70	+ 0·22
28·595862	+ 14 40 59·50	+ 5·93	. .	— 0·19	— 0·59	+ 2·00	6·65	4·61	— 2·04
Nov. 19·671401	+ 14 49 30·70	+ 4·44	. .	+ 0·01	+ 0·16	— 0·61	34·70	37·62	+ 2·92

Notes.—Oct. 9, *Dun Echt.* The place of the comet has been taken from No. 14 of "Copernicus," which differs somewhat from that given in Ast. Nach. No. 2398. Oct. 26, *Palermo.* The coordinates given in Ast. Nach. No. 2403 are founded on a place of 37 Leonis which is that given in the catalogue on page 7. If the corrected place for 37 Leonis be used, the resulting coordinates of the comet are $\alpha = 10^h\ 8^m\ 37^s{\cdot}25$ and $\delta = 14^\circ\ 43'\ 6''{\cdot}31$. Subsequently Dr Zona determined the place of his comparison star directly to be $\alpha = 10^h 8^m 4^s{\cdot}60$ $\delta = +\ 14^\circ\ 34'\ 35''{\cdot}5$, whence the position of the comet is found to be $\alpha = 10^h\ 8^m\ 37^s{\cdot}21$ $\delta +\ 14^\circ\ 43'\ 8''{\cdot}53$. The mean of these two values, namely, $\alpha = 10^h\ 8^m\ 37^s{\cdot}23$ $\delta = 14^\circ\ 43'\ 7''{\cdot}42$ has been taken as the most probable position of the comet.

Oct. 21 and Nov. 3, *Rome.* The places are taken from MSS. observations kindly supplied by Profs. Tacchini and Millosevich.

Oct. 19, *Strassburg.* Dr. Winnecke has not given the adopted place of his comparison star. It is assumed that Bessel's place has been used unaltered.

Supposing the corrections to the Ephemeris to be expressed by an equation of the form $\Delta\theta = \mathrm{A} + \mathrm{B}t + \mathrm{C}t^2$, and giving to all the observations equal weight, since the data for a more homogeneous combination does not exist in all cases as already mentioned, I have obtained the following expressions for the corrections to each of the spherical coordinates—

$$\begin{aligned}\Delta\alpha \cos\delta &= +\ 9''{\cdot}52\ +\ 1''{\cdot}2467\ t - 0''{\cdot}1066\ t^2\\ \Delta\delta &= +\ 0{\cdot}465 +\ 0{\cdot}3223\ t -\ 0{\cdot}0136\ t^2\end{aligned}$$

and from these expressions I have computed the corrections for Greenwich midnight, for every fourth day from Oct. 8 to Oct. 28, and used the result so obtained for the independent term in the equations of condition for those dates. For Nov. 3, the determination of the place of the comet is a matter of considerable difficulty. From the three observations on that day we have—

	$d\alpha \cos\delta$	$d\Delta$
Marseilles	$+\ 2''{\cdot}59$	$+\ 3''{\cdot}06$
Athens	$-\ 42{\cdot}02$	$+\ 28{\cdot}42$
Rome	$-\ 17{\cdot}49$	$+\ 5{\cdot}65$

Dr. Schmidt, however, places but little reliance on his own observation, and since the large negative correction given by the observation at Rome is entirely unsupported by the observation of Oct. 30 at Paris, I have decided to reject that observation also. For Declination, however, the mean of the errors given by the Rome and Marseilles observations seems unobjectionable. On Nov. 19 the Strassburg observation is, of course, used unaltered. A weight of ½ has been arbitrarily given to the last two equations of condition.

Before giving the normal equation computed in the manner here described, it may be desirable to compare the errors of the Ephemeris deduced from the equations given above with those derived from grouping the observations at short intervals. The agreement between the two results is not so close as could have been wished, but it must be remembered that the mean of the groups rests on very few observations, and that by transferring an observation from one group to the next a very considerable alteration can be made in the mean—

Date.	No. of Observations in the Group.	Δα cos δ (Mean).	Δα cos δ from the Equation.	Difference.	Δδ (Mean).	Δδ from the Equation.	Difference.
Oct. 9·94	6, 6	+ 15″·10	+ 11″·76	+ 3″·34	− 0″·52	− 0″·47	+ 0″·05
13·66	2, 2	+ 11·21	+ 12·81	− 1·60	+ 0·93	+ 0·54	− 0·39
18·35	6, 6	+ 11·37	+ 13·13	− 1·76	+ 0·11	+ 1·27	+ 1·16
21·39	6, 5	+ 13·28	+ 12·69	+ 0·59	+ 1·88	+ 1·43	− 0·45
26·40	6, 5	+ 13·54	+ 10·91	+ 2·63	+ 2·70	+ 1·13	− 1·57
29·30	3, 3	+ 8·03	+ 9·26	− 1·23	− 0·56	+ 0·65	+ 1·21

The equations which express the correction to the Ephemeris have also been used to determine the mean and probable errors of a single observation, and have thus furnished a rule for the rejection of discordant observations. The observations are, of course, not sufficiently numerous to warrant the strict application of any formulæ, founded upon the general principles of the distribution of errors in an unlimited number of observations, but the employment of such formulæ furnishes a criterion for the retention of doubtful observations more trustworthy than any other. And, moreover, in the future comparisons instituted between places derived from orbits differing but slightly from each other, and the observations themselves, we have a guide as to the possible degree of accuracy that may be expected, and a more trustworthy guide for the rejection of deduced elements. Excluding for the present the observation of Nov. 19 at Strassburg, which is so far distant in point of time from the rest of the series that the application of the equations given above for the correction of the Ephemeris is not to be trusted, and rejecting some of the observations which are grossly discordant, it appears that the sum of the squares of the residuals in R.A. and Declination is 2444″·36, and 1123″·18 respectively, and consequently the probable error of a single observation is in each of the spherical coordinates 5″·89 and 4″·12 respectively, and the limiting value for the rejection of discordant positions of the comet is 21″·11, and 14″·72 in the two elements. Errors exceeding or approaching the amount will, therefore, in the future, not be taken into consideration.

It will be sufficient to give here the normal equations from the solution of which the corrections to the assumed elements have been derived, since it has already been explained how the independent term in the several equations of condition has been formed, and the differential coefficients have been taken directly from the same source as that employed in the calculation of the perturbations. After treatment of the equations of condition by the ordinary method and putting $\mu' = 100\mu$, the final equations become—

$$
\begin{array}{rrrrrrrl}
+35''\cdot0838 & +1\cdot89289d\pi & -0\cdot07797d\Omega & +0\cdot13122di & +10\cdot05659d\phi & -0\cdot64148dM & +11\cdot11694d\mu' & =0 \\
-4\cdot2519 & -0\cdot07797 & +0\cdot10209 & -0\cdot26885 & -0\cdot27796 & -0\cdot90056 & -0\cdot14814 & =0 \\
+9\cdot5325 & +0\cdot13122 & -0\cdot26885 & +1\cdot09489 & +0\cdot43684 & +1\cdot72237 & +0\cdot17704 & =0 \\
+220\cdot4474 & +10\cdot05659 & -0\cdot27796 & +0\cdot43684 & +57\cdot98643 & -8\cdot61199 & +64\cdot56549 & =0 \\
-15\cdot4324 & -0\cdot64148 & -0\cdot90056 & +1\cdot72237 & -8\cdot61199 & +13\cdot46470 & -11\cdot40859 & =0 \\
+240\cdot4523 & +11\cdot11694 & -0\cdot14814 & +0\cdot17704 & +64\cdot56549 & -11\cdot40859 & +72\cdot18781 & =0
\end{array}
$$

The last equation is so nearly allied to the fourth that the solution of the general equation by the ordinary method of substitution is eminently insecure. The solution, however, gives the following results—

$$\begin{aligned} d\pi &= +\ 63''\cdot107 \\ d\Omega &= +\ 409\cdot284 \\ di &= +\ 48\cdot360 \\ d\phi &= +\ 286\cdot555 \\ dM &= -\ 20\cdot411 \\ d\mu' &= -\ 272\cdot09 \end{aligned}$$

The new elements that result from the application of the corrections are as follow :—

$$\begin{array}{rlll} T = & \text{Sept. } 28\cdot5 \text{ Greenwich Mean Time.} & & \\ M = & 1^\circ\ \ 40'\ \ 49''\cdot35 & & \\ \pi = & 18\ \ 34\ \ 54\cdot1 & \left.\vphantom{\begin{array}{l}1\\1\\1\end{array}}\right\} & \text{M. Eq. } 1881\cdot0. \\ \Omega = & 66\ \ \ 1\ \ \ \ 4\cdot1 & & \\ i = & 6\ \ 51\ \ 31\cdot8 & & \\ \phi = & 56\ \ 11\ \ 51\cdot0 & & \\ \mu = & \qquad 398\cdot9608 & & \end{array}$$

The positions of the comet computed from these elements, and the positions inferred from the substitution of the corrections to the elements in the original equations of condition agree generally, but owing to the considerable amount of the corrections, and the neglect of the terms of the second order in computing the differential coefficients the agreement is not exact. The accompanying table shows the corrections to the originally computed places derived from both sources—

Date.	$\Delta\alpha \cos\delta$, derived from Equations	from Elements.	$\Delta\delta$, derived from Equations	from Elements.
October 8·5	− 23″·50	− 26″·30	+ 1″·69	+ 2″·30
12·5	− 16·31	− 17·79	− 1·41	− 1·40
16·5	− 11·54	− 12·08	− 3·18	− 1·80
20·5	− 8·58	− 9·96	− 3·87	− 2·90
24·5	− 7·08	− 6·19	− 3·87	− 3·80
28·5	− 6·34	− 7·45	− 4·18	− 4·60
November 3·5	− 2·69	− 2·16	− 1·10	− 1·50
19·7	− 7·58	− 9·54	+ 4·30	+ 5·10

If either of these results be compared with the quantities given by the equations—

$$\begin{aligned} \Delta\alpha \cos\delta &= +\ 9''\cdot52 + 1''\cdot2467\ t - 0''\cdot1066\ t^2 \\ \Delta\delta &= +\ 0\cdot465 +\ 0\cdot3223\ t - 0\cdot0136\ t^2 \end{aligned}$$

or if the corresponding quantities be interpolated for the times Oct. 9·94, Oct. 13·66, &c., and compared with the table given on page 12, the agreement is scarcely satisfactory, so that either the corrections to the elements are untrustworthy, or the errors of the normal places are considerable. To determine the degree of confidence to be placed on the corrections given above, it will scarcely be sufficient to derive the weights and probable errors of the corrections in the usual manner, since the mean error of an equation of condition of the weight unity cannot be accurately determined. Since the principal object of the investigation is the determination of the mean motion, it seems more prudent to obtain the values of the several unknown quantities in terms of the mean motion, when by successive variations of this element accompanied by the necessary alterations of the others, several sets of elements can be derived, and the most probable selected by actual comparison with the observations themselves. Treating the normal equations by the method of indeterminable multipliers as an additional check on the accuracy of solution, the following corrections have been obtained—

$$\begin{aligned} d\pi &= +\ 192''{\cdot}001 + 0{\cdot}47374\ d\mu' \\ d\Omega &= -\ \ 478{\cdot}335 - 3{\cdot}27509\ d\mu' \\ di &= -\ \ \ 64{\cdot}742 - 0{\cdot}41458\ d\mu' \\ d\phi &= -\ \ \ 45{\cdot}192 - 1{\cdot}21925\ d\mu' \\ dM &= -\ \ \ 42{\cdot}323 - 0{\cdot}07501\ d\mu' \end{aligned}$$

The correction to μ was then supposed to be successively zero, $\pm\ 1''$ and $\pm\ 2''$, and new sets of elements having been formed, ephemerides were recomputed directly from the elements, and the observations compared with each in turn. The results of the comparison are given in the next table which is sufficiently explained by the headings of the columns.

Date.	$\mu + 2''$		$\mu + 1''$		μ		$\mu - 1''$		$\mu - 2''$	
	$\Delta\alpha \cos\delta$	$\Delta\delta$	$\Delta\alpha \cos\delta$	$\Delta\delta$	$\Delta\alpha \cos\delta$	$\Delta\delta$	$\Delta\alpha \cos\delta$	$\Delta\delta$	$\Delta\alpha \cos\delta$	$\Delta\delta$
Oct. 5·69	+ 8″·97	−21″·20	+ 3″·05	−17″·21	− 4″·47	−12″·80	−12″·73	− 6″·53	−20″·69	− 1″·21
9·70	+ 11·53	− 3·45	+ 9·12	− 2·93	+ 6·47	− 1·56	+ 3·61	+ 0·54	− 0·38	+ 2·25
10·67	+ 8·97	− 1·17	+ 7·51	− 0·96	+ 5·66	− 0·24	+ 3·52	+ 0·88	+ 1·15	+ 1·95
10·86	+ 7·76	− 7·96	+ 6·40	− 7·79	+ 4·74	− 7·19	+ 2·65	− 6·25	+ 0·48	− 5·29
10·87	+ 10·59	− 1·36	+ 9·24	− 1·16	+ 7·58	− 0·57	+ 5·49	+ 0·37	+ 3·34	+ 1·83
11·84	+ 5·48	− 7·37	+ 6·12	− 7·60	+ 7·51	− 7·37	+ 8·88	− 7·29	+ 10·40	− 6·94
12·63	− 2·73	+ 2·69	− 2·55	+ 2·26	− 3·42	+ 2·09	− 3·77	+ 1·69	− 5·12	+ 1·48
14·70	− 5·27	+ 1·71	− 4·31	+ 0·36	− 3·64	− 1·01	− 2·93	− 2·32	− 3·08	− 3·26
17·61	− 7·52	+ 2·94	− 5·97	+ 0·59	− 4·52	− 1·62	− 3·05	− 3·53	− 3·42	− 5·41
17·67	− 6·58	+ 10·87	− 5·03	+ 8·47	− 3·60	+ 6·25	− 2·10	+ 4·28	− 0·54	+ 2·45
18·61	− 8·54	+ 8·00	− 6·91	+ 5·79	− 5·25	+ 3·45	− 3·51	+ 0·99	− 1·86	− 0·89
18·62	+ 4·68	+ 3·05	+ 6·30	+ 0·84	+ 7·97	− 1·50	− 9·72	− 3·96	+ 11·37	− 5·84
18·68	− 14·58	+ 6·17	− 12·97	+ 3·94	− 11·29	+ 1·63	− 9·55	− 0·82	− 7·89	− 2·74
18·90	− 0·01	+ 7·61	+ 1·62	+ 5·38	+ 3·37	+ 3·00	+ 5·13	+ 0·49	+ 6·85	− 1·39
19·67	− 11·02	+ 7·06	− 9·29	+ 4·65	− 7·62	+ 2·28	− 5·58	− 0·27	− 3·87	− 2·24
20·89	+ 0·54	+ 7·15	+ 2·75	+ 4·65	+ 4·74	+ 2·15	+ 6·61	− 0·41	+ 8·21	− 2·46
21·62	− 3·51	+ 17·87	− 1·12	+ 15·18	+ 0·94	+ 12·65	+ 2·80	+ 10·13	+ 4·52	+ 8·01
21·65	− 2·79	+ 5·27	− 0·40	+ 2·45	+ 1·67	+ 0·02	+ 3·58	− 2·40	+ 5·33	− 4·62
21·86	− 1·52	+ 8·20	+ 0·95	+ 2·50	+ 3·05	+ 2·97	+ 4·85	+ 0·45	+ 6·60	− 1·67
22·62	− 8·29	. .	− 5·54	. .	− 3·33	. .	− 1·64	. .	+ 0·20	. .
25·57	− 16·03	+ 9·10	− 12·56	+ 6·15	− 10·24	+ 3·84	− 9·27	+ 1·79	− 7·43	− 0·32
25·57	− 7·89	+ 20·50	− 4·42	+ 17·55	− 2·10	+ 15·24	− 1·13	+ 13·19	+ 0·72	+ 11·08
25·87	+ 12·76	+ 12·37	+ 16·16	+ 9·49	+ 18·45	+ 7·22	+ 19·38	+ 5·24	+ 21·19	+ 3·13
26·60	− 6·29	+ 25·12	− 2·98	+ 24·44	− 0·91	+ 20·25	− 0·08	+ 18·38	+ 1·59	+ 16·38
26·63	− 24·28	. .	− 20·98	. .	− 18·90	. .	− 18·08	. .	− 16·12	. .
26·90	− 7·92	+ 5·63	− 4·74	+ 3·07	− 2·70	+ 0·90	− 1·87	− 0·90	− 0·31	− 2·86
27·88	+ 6·36	− 1·81	+ 8·41	− 3·91	+ 10·19	− 5·93	+ 11·12	− 7·56	+ 12·43	− 9·34
28·60	− 13·18	+ 2·74	− 11·72	+ 0·66	− 10·38	− 1·16	− 9·21	− 2·66	− 8·15	− 4·24
28·65	− 2·61	+ 9·12	− 0·24	+ 7·04	+ 1·14	+ 5·23	+ 2·31	+ 3·75	+ 3·40	+ 2·16
30·65	+ 4·84	− 1·02	+ 4·77	− 2·55	+ 4·48	− 3·89	+ 4·56	− 5·03	+ 4·84	− 5·99
Nov. 3·68	. .	+ 4·58	. .	+ 4·34	. .	+ 4·14	. .	+ 3·96	. .	+ 4·09
3·69	− 2·39	+ 1·99	− 3·34	+ 1·75	− 3·59	+ 1·55	− 3·66	+ 1·37	− 4·54	+ 1·50
19·67	+ 8·68	− 21·80	+ 2·49	− 14·94	− 2·02	− 8·78	− 7·56	− 2·34	− 13·64	+ 4·28

The sum of the squares of the residuals resulting from the employment of each of these five hypotheses is as follows :—

	$\Delta\alpha^2 \cos^2 \delta$	$\Delta\delta^2$	Sum.
$\mu + 2''$	2660″·7	3317″·9	5978″·6
$\mu + 1$	1912·5	2250·9	4163·4
μ	1689·3	1409·7	3099·0
$\mu - 1$	1785·4	972·5	2757·9
$\mu - 2$	2252·0	815·8	3067·8

The deduction of the correction to the mean motion by this process is not as encouraging as was anticipated. The minimum sum of the squares derived from the two elements does not correspond to the same value of μ. From the observations of Right Ascension alone, a correction to the mean motion of only $-$ 0″·245 would be required, while from the declination observation the correction required is more than $-$ 2″. This gradual decrease in the declination errors is to some extent explained by the retention of a few observations affected with evidently large positive errors, which though gradually removed by a decrease in the mean motion have nevertheless a disproportionate influence on the sum of the squares. Though the sum of the squares is numerically less in the case of the last hypothesis than in any of the others, yet in 19 cases out of 31 the error has been increased, indicating that the general minimum was reached when a correction of less than $-$ 2″ was applied to μ. Adopting the sum of the squares of the errors in the two elements as the more trustworthy guide, the correction to μ is $-$ 1″·06. This correction differs very considerably from that given on page 13, but is probably a closer approximation. An alteration in the mean daily motion of 1″·7 is attended with an increase in the probable error of a single observation of one second of arc, but an alteration of this amount is shown by the last table to be inadmissible.

If we accept as final this latter correction of $-$ 1″·06 to the mean daily motion, the probable error of a single observation in Right Ascension and Declination is 5″·14 and 3″·81 respectively. These quantities are less than those obtained on page 12 from the corrections to the ephemeris, but this arises from excluding some of the more erroneous observations as has been already explained. The resulting osculating elements become as follow :—

$$
\begin{array}{rl}
T = & 1881, \text{ Sept. } 28{\cdot}5 \text{ Greenwich Mean Time.}\\
M = & 1^\circ\ \ 40'\ \ 35''{\cdot}39\\
\pi = & 18\ \ \ 36\ \ \ 12{\cdot}78\\
\Omega = & 65\ \ \ 52\ \ \ \ \ 2{\cdot}04\\
i = & 6\ \ \ 50\ \ \ 22{\cdot}61\\
\phi = & 56\ \ \ \ \ 8\ \ \ 28{\cdot}42\\
\log a = & 0{\cdot}6315148
\end{array}
\left.\begin{array}{l} \\ \\ \\ \\ \\ \\ \\ \end{array}\right.
$$

π, Ω, i: Mean Eq. 1881·0.

Period 3235 days.

The circumstances of the next return can be more conveniently considered after the publication of the ephemerides of the planets.

UEBER DEN ZWEITEN DIFFERENTIALQUOTIENTEN VON x NACH DER WAHREN ANOMALIE GENOMMEN.

Es sei x eine Funktion von t, welche in x_{-1} oder in x_{+1} übergehe, je nachdem $t - dt$ oder $t + dt$ an der Stelle von t gesetzt wird. Nach dem Taylor'schen Satze hat man dann die Gleichungen—

$$x_{-1} = x - \frac{dx}{dt}\,dt + \tfrac{1}{2}\,\frac{d^2x}{dt^2}\,dt^2 - \ldots \tag{1}$$

$$x_{+1} = x + \frac{dx}{dt}\,dt + \tfrac{1}{2}\,\frac{d^2x}{dt^2}\,dt^2 + \ldots \tag{2}$$

Durch Addiren erhält man—

$$x_{-1} + x_{+1} = 2x + \frac{d^2x}{dt^2}\,dt^2 + \ldots \tag{3}$$

Die Beziehung, welche zwischen der Zeit t und der wahren Anomalie v besteht, ist bekanntlich durch die Differentialgleichung—

$$\frac{dv}{dt} = \frac{k\sqrt{1+m}\,\sqrt{p}}{r^2}$$

ausgedrückt. Denkt man sich x als Funktion von v dargestellt, und setzt man alsdann an die Stelle von v nach einander die Werthe $v - dv$ und $v + dv$, so geht x über in X_{-1} und X_{+1} und es bestehen die Gleichungen—

$$X_{-1} = x - \frac{dx}{dv}\,dv + \tfrac{1}{2}\,\frac{d^2x}{dv^2}\,dv^2 - \ldots \tag{4}$$

$$X_{+1} = x + \frac{dx}{dv}\,dv + \tfrac{1}{2}\,\frac{d^2x}{dv^2}\,dv^2 + \ldots \tag{5}$$

Durch die Addition entsteht—

$$X_{-1} + X_{+1} = 2x + \frac{d^2x}{dv^2}\,dv^2 + \ldots \tag{6}$$

Wenn nun die Bestimmung getroffen wird, dass in diesen Gleichungen die Beziehung zwischen dt und dv durch die Gleichung—

$$\frac{dv}{dt} = \frac{k\sqrt{1+m}\,\sqrt{p}}{r^2}$$

ausgedrückt sei, so ist zwar $X_{-1} = x_{-1}$ und $X_{+1} = x_{+1}$, aber nur so lange als man sich auf die Glieder der nullten und ersten Ordnung beschränkt. Wenn auch die Glieder der zweiten Ordnung berücksichtigt werden sollen, so ist nicht mehr $X_{-1} = x_{-}$ und $X_{+1} = x_{+1}$. Wir haben aus (1) und (2) die Glieder der ersten

Ordnung eliminirt und die Gleichung (3) erhalten, und ebenso aus (4) und (5) die Gleichung (6). Eliminirt man aus (3) und (6) auch die Glieder der nullten Ordnung, so entsteht—

$$X_{-1} + X_{+1} - x_{-1} - x_{+1} = \frac{d^2x}{dv^2}\,dv^2 - \frac{d^2x}{dt^2}\,dt^2 :$$

Wollte man hier $X_{-1} = x_{-1}$ und $X_{+1} = x_{+1}$ setzen, so würde man die Gleichung—

$$\frac{d^2x}{dt^2} = \frac{d^2x}{dv^2}\left(\frac{dv}{dt}\right)^2 \qquad (\alpha)$$

erhalten, was nicht richtig ist.

In No. 21 dieser Zeitschrift (p. 164) wird behauptet, dass die Gleichung (α) eine identische sei, wiewohl die Analysis lehrt, dass die Gleichung—

$$\frac{d^2x}{dt^2} = \frac{d^2x}{dv^2}\left(\frac{dv}{dt}\right)^2 + \frac{dx}{dv}\,\frac{d^2v}{dt^2} \qquad (\beta)$$

anzuwenden ist, wenn die unabhängige Veränderliche t durch die neue Veränderliche v ersetzt werden soll. Dass die Gleichungen $X_{-1} = x_{-1}$ und $X_{+1} = x_{+1}$ nicht mehr richtig sind, wenn die Glieder der zweiten Ordnung in Betracht kommen, ist im Grunde von selbst einleuchtend. Wenn dies aber nicht ganz verständlich zu sein scheint, kann man sich leicht davon überzeugen, dass die Gleichung (α) unrichtig ist, indem man irgend eine bestimmte Funktion von t an die Stelle von x setzt. Ich setze in dieser Absicht $x = t$, und die linke Seite der Gleichung (β) ist $\frac{d^2x}{dt^2} = 0$ Die endliche Gleichung zwischen den Veränderlichen t und v ist bekanntlich—

$$k\sqrt{\frac{1+m}{a^3}}\,(t - t_0) = E - e \sin E, \text{ wo } \operatorname{tang} \tfrac{1}{2} E = \sqrt{\frac{1-e}{1+e}}\,\operatorname{tang} \tfrac{1}{2} v$$

ist. Durch Differentiation findet man die oben erwähnte Gleichung—

$$\frac{dv}{dt} = \frac{k\sqrt{1+m}\sqrt{p}}{r^2} = k\sqrt{\frac{1+m}{p^3}}\,(1 + e \cos v)^2,$$

durch die zweite Differentiation entsteht—

$$-\frac{d^2v}{dt^2} = 2k\sqrt{\frac{1+m}{p^3}}\,(1 + e \cos v)\, e \sin v\,\frac{dv}{dt} = 2k^2\,\frac{1+m}{p^3}\,(1 + e \cos v)^3\, e \sin v.$$

Fur $x = t$ erhält man ferner die Gleichungen—

$$\frac{dx}{dv} = \frac{1}{k}\sqrt{\frac{p^3}{1+m}}\,\frac{1}{(1 + e \cos v)^2}$$

$$\frac{d^2x}{dv} = \frac{1}{k}\sqrt{\frac{p^3}{1+m}}\,\frac{2\, e \sin v}{(1 + e \cos v)^3}$$

Die rechte Seite der Gleichung (β) geht daher über in—

$$\frac{1}{k}\sqrt{\frac{p^3}{1+m}}\,\frac{2\,e\sin v}{(1+e\cos v)^3}\,k^2\,\frac{1+m}{p^3}\,(1+e\cos v)^4$$
$$-\frac{1}{k}\sqrt{\frac{p^3}{1+m}}\,\frac{1}{(1+e\cos v)^2}\,2k^2\,\frac{1+m}{p^3}\,(1+e\cos v)^3\,e\sin v=0$$

Die Gleichung (β) ist also eine identische, und daraus folgt, dass die Gleichung (α) nicht identisch ist. Es ist damit freilich nicht die allgemeine Gültigkeit der Gleichung (β) bewiesen; allein es ist doch durch die Betrachtung des Beispiels $x = t$ die Glaubwürdigkeit desjenigen, was in den Elementen der Analysis gelehrt wird, in ein hinreichend helles Licht gestellt.

August Weiler.

Karlsruhe, den 27 October, 1882.

OBSERVATION OF THE TRANSIT OF VENUS, 1882, DECEMBER 6, AT THE ARMAGH OBSERVATORY.

By J. L. E. DREYER.

On the morning of the 6th of December the weather did not look very promising, and between nine and ten o'clock snow fell in considerable quantities. The sky cleared, however, shortly after eleven o'clock, and from that moment, and up to about three o'clock, the weather remained as fine as could be desired, a few light clouds only passing now and then, but fortunately not at any of the critical moments.

Although the mirror of the 15-inch Reflector (made by the late Mr. Thomas Grubb, and mounted equatoreally under the East dome) is at present very much in need of being polished, its figure is nearly perfect. I procured from Mr. Howard Grubb a plane unsilvered mirror to take the place of the plane metallic mirror otherwise used when the telescope is employed in the Newtonian form,* also a negative eye-piece (power 140) furnished with a wedge of neutral tinted glass which enables an observer to modify the intensity of the sun's light so as to make it bearable to the eye without obliterating the granular appearance of the photosphere. The eye-piece was carefully focussed both on the solar limb and on various faculæ, and in moments of good definition (especially after stopping down the aperture to seven inches) the limb and surface-details were very sharply defined.

When the moment of external contact drew near, I put on a diaphragm of 11-in. aperture, but the limb of the sun was *boiling* so violently that the exact time of external contact could not be observed. I only noted that at $1^h\ 36^m\ 14^s$ Local

* It can also be used in the Cassegrain form.

Mean Time it was certainly passed. I then changed the diaphragm for one of seven inches aperture, and the definition became in consequence greatly improved. At $1^h\ 49^m\ 31^s$ the whole circumference of Venus was distinctly visible. I observed the internal contact as follows:—

(1) $1^h\ 54^m\ 49^s{\cdot}6$ A shade is seen to form between the cusps, the limb of the planet apparently being in contact with the prolongation of the cusps.
(2) $1\ \ 55\ \ 24{\cdot}5$ Shade broke suddenly, and a thin line of light separates Venus from the sky outside.

At $1^h\ 55^m\ 51^s$ I noted "Interval between limbs very conspicuous now." This only means that the interval could be seen at a glance without special attention being directed to it. It had, however, been steadily visible from $1^h\ 55^m\ 24^s{\cdot}5$.

The moments (1) and (2) evidently refer to the phenomena generally called "apparent" and "real contact." The third figure on Plate XIV. of the volume of British Observations of the Transit of 1874 (Observations by Commander Wharton at Rodriguez) represents nearly what I saw just before the breaking of the "shade."

The transit was observed by the Rev. Charles Faris, M.A., with the 3·8-inch achromatic finder of 7½ feet focal length, attached to the tube of the Reflector. As we were hidden from one another by the pier of the instrument and neither of us spoke a word or stirred from our places during the Ingress, our observations are perfectly independent. Mr. Faris did not observe any "shade" or "black drop;" he simply observed—

External contact,	1^h	35^m	35^s
Internal do.,	1	55	10·6

Time determinations were obtained on the evenings of December 5 and 6. The longitude of the Armagh Observatory was determined by the late Dr. Robinson as follows (Trans. Roy. Irish Acad. Vol. XIX.)—

By two Solar Eclipses and 19 occultations, ...	$26^m\ 35^s{\cdot}58$
By 15 chronometers (in 1838), ...	35·44
By Lunar Transits, ...	35·64
By rocket signals between Armagh and Dunsink, assuming the long. of Dunsink as found by the 15 chronometers in 1838 = $25^m\ 21^s{\cdot}08$, .	35·34

It may here be mentioned that the longitude of Dunsink, as given in the Nautical Almanac, is nearly a whole second too great.

THE TRANSIT OF VENUS OBSERVED AT MARKREE OBSERVATORY.

By W. DOBERCK.

While cloudy skies generally prevail here even when the weather is fine in other parts of the British islands, we enjoyed a splendid day on the 6th of December, 1882, while the weather was unfavourable in England and even in the

Eastern counties of Ireland. I observed the Transit of Venus in the 25-feet Refractor, the aperture of which I had reduced to five inches, with the Munich filar micrometer, the eye-piece having been accurately focussed on Double Stars on the previous evening. I had fixed a dark orange glass with bee's wax to the eye-piece (power about 130) and kept in my hand a light blue glass, usually employed for looking at the Moon or at bright clouds. This blue glass I used whenever the Sun was quite free from clouds. The definition shortly before Ingress was bad and owing to the boiling of the Sun's limb I did not remark this phenomenon till $1^{h}\ 28^{m}\cdot0$ ± when the outer contact had already taken place. Shortly afterwards I began to measure the distance between the cusps and obtained the following results—

Markree M.T.	Distance.
$1^{h}\ 34^{m}\ 38^{s}$	$6^{r}\cdot461$
1 36 38	6·659
1 37 53	7·090
1 39 8	6·842
1 40 38	6·731
1 42 8	6·182
1 43 53	5·579

From these measures it appears that the planet was bisected at $1^{h}\ 38^{m}\ 0^{s}$ or thereabouts. I estimated the bisection to take place at $1^{h}\ 35^{m}\ 38^{s}$, but as Venus had the shape of a half ellipse, this is of course very uncertain. At $1^{h}\ 46^{m}\ 40^{s}$ I could see the outline of Venus beyond the Sun's edge, a fine white line appearing to surround it. At 1 $47^{m}\ 58^{s}$ the cusps met for one instant but did not join permanently till $1^{h}\ 48^{m}$ 28 . I did not see any "black drop." After this moment Venus appeared to me as a perfectly dark hole in the Sun, the diameter of which could not easily be measured with accuracy, as the light of the Sun gradually increased round it, partly owing to diffraction and bad definition, partly owing to the atmosphere of the planet. The latter, no doubt, was also the cause of the faint light seen round the limb of the planet before Ingress.

I measured the horizontal diameter of the perfectly black body of the planet from $2^{h}\ 11^{m}$ to $2^{h}\ 20^{m}$ and from five measures of the double diameter I found the following values for the semi-diameter—

$3^{r}\cdot430$
3·336
3·357
3·266
3·379

Mean 3·354 = $29''\cdot11$

Markree Observatory,
1882 Dec. 12.

RECENT LITERATURE.

"*Areographische Beiträge zur genauern Kenntniss und Beurtheilung des Planeten Mars. Von Dr. J. H. Schröter. Nach dem Manuscripte auf der Leidener Sternwarte herausgegeben von H. G. van de Sande Bakhuyzen.* Leiden 1881,[*] 8vo, 447 pp. and 16 plates of engravings. The name of Schröter has of late years had a much better sound, and his observations have been far oftener referred to, than was the case some forty or fifty years ago, when Mädler in particular lost no opportunity to attack them. At present, when so many observers are watching the physical appearance of the larger planets, and we possess such an abundance of drawings of these, it is of very great interest to examine and discuss the drawings and notes made with scrupulous care and great perseverance by Schröter nearly a century ago. Since 1873, when Dr. Terby published an account of Schröter's observations of Mars, astronomers have been aware that there still existed a bulky manuscript entitled the "Areographische Beiträge" which had been rescued from the thorough destruction of Schröter's house and observatory by the retreating French army in 1813.[†] In 1876 the Leiden Observatory purchased the manuscript and the fourteen copper plates which Schröter had caused to be engraved, and after having had two missing plates engraved from the original drawings, Professor Bakhuyzen has now published Schröter's work in exactly the shape as Schröter would have done, nearly two-thirds of the M.S. having been finally revised for the press by the Author.

The book is divided into nine chapters, the eight of which contain the observations, measures and notes, from 1785 to 1803, while the ninth contains a discussion of the conclusions which Schröter draws from them as to the physical constitution of Mars. The greatest number of observations were made in 1798, when Mars was in perihelion nine days before the Opposition. Already from 1788 Schröter had tried to determine the ellipticity but could not find a trace of deviation from a perfect circle. In 1792 he found the ellipticity = $\frac{1}{15}$ (Herschel had found $\frac{1}{16}$), but the position of the major axis did not agree with the direction of the motion of the spots. In 1798 he again found the ellipticity insensible. The diameter he found = 9″·8. All Schröter's measures were made with his "Projection-machine," and Herschel's "Lamp Micrometer" (described in the first volume of Schröter's Beiträge), which employed both eyes and were capable of more precision in the hands of a skilful observer than many might be inclined to think. The position of the axis of rotation was also determined by Schröter in 1798, and

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[*] Published in July, 1882.

[†] Though Schröter, who was Hanoverian Ober Amtmann in Lilienthal near Bremen, had founded his Observatory at his own expense, the whole of the Instruments were bought by the Government in 1800, reserving to S. the entire use of them for lifetime. The Government also paid his Assistant (Harding till 1805, then Bessel till 1810). Zach has given a full description of the buildings in the Monatl. Corresp. III. p. 479 & seq. The 27-feet reflector (which turned round the eye-piece) is figured in the Aphroditographische Fragmente. After Schröter's death his instruments became the property of the Göttingen Observatory.

Professor Bakhuyzen, who has verified all the computations in the book, on going over the measures again, found for the Longitude and Latitude of the axis 352° 59′ and 60° 32′ in good accordance with W. Herschel's results, but differing more from those of Schiaparelli.

The great mass of Schröter's observations and notes refer however to the spots on Mars, and the very full manner in which the most minute details are given have made the book (like all Schröter's publications) swell to a considerable size. From the beginning and throughout the progress of his observations one cannot help wondering at the tenacity with which Schröter clings to his preconceived idea, that the spots are nothing but clouds in the Martian atmosphere, and how he was never shaken in this opinion even when seeing the same spot appear in the same place for several months. He very often finds the motion of the spots disagreeing with the period of rotation and in the last chapter he even puts together an "Anemometrical table" of the prevailing winds, but in most cases we believe it will be found possible to interpret the observations without having recourse to a physical motion of the spots. In the first place Schröter seems to have overlooked the influence of indistinct seeing, especially towards the limbs, and secondly he has often made mistakes in identifying spots seen with an interval of some days. Another result of such mistakes is a number of supposed changes in the spots, but so long as the Longitudes of the central meridian for the date of each observation has not been computed, it is difficult to test the accuracy of each case.* A careful reader who is acquainted with the configurations on the surface of Mars will however in many cases be able to see that there really was no change, as for instance on Sept. 12, 1798, when successively *Kaiser See* and *Herschel II Strait* were seen. It is however not impossible that even a close scrutiny will fail to interpret all the supposed changes in this way and that future Areographers will have to deal with some of them as established facts. But as we have of late heard so much of the variability of the "canals" and other minor details, it would after all not be so very surprising if also some of the larger markings were found to be less permanent than generally supposed.

Professor Bakhuyzen deserves our best thanks for having at last given to the world this great mass of observations and for his care in revising them. He justly remarks that the very circumstance that Schröter believed the markings to be variable, prevented him from being biassed when drawing the same spot repeatedly. Though doubtless many of Schröter's views were erroneous, his own writings and the veneration with which men like Olbers and Bessel always spoke of him cannot fail to give every unprejudiced mind the impression that his observations are well worth studying, and that among the number of great names in Astronomy around the year 1800 that of Schröter deserves an honoured place.

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* We understand that Prof. Bakhuyzen is about to publish a list of these Longitudes.

Annals of the Observatory of Harvard College, Vol. XIII. Part I. Micrometric Measurements, 1866–81. Cambridge U.S. 1882, 204 pp. 4to. The observations recorded in this volume were all made with the 15-inch refractor and filar micrometer. Chapter I. contains a full account of the investigations on the micrometer, including an exact determination of the focal length of the 15-inch object-glass. Then follow 1019 single night's results of measures of Double Stars (most of them from the years 1866–72), a list being also given of 109 new objects found at Harvard College, including some stars only suspected to be double and some which have since been found independently by Mr. Burnham. These discoveries were chiefly made during an exploration of the southern heavens carried on in the intervals of other work. Some parts of the region north of − 2° Decl. having also been explored, a list is added of all D.M. stars in the zones − 1° to + 8° which have been examined and found single. The third chapter contains 400 single observations of Nebulæ, including some merely descriptive ones and some spectroscopic observations. Among the nebulæ are thirteen not previously recorded. A note on the Trifid Nebula gives a detailed comparison between Trouvelot's positions of 104 stars in or about the nebula (see Vol. VIII. of the Annals) and the positions given by other observers. The measures of the Satellites of Saturn, Uranus and Neptune (Chapter IV.) are from the years 1866–68 but have not been printed before. Observations of the satellites of Mars (Chapters V. and VI.) were made both in 1877 and in 1879. As in the latter year the satellites were expected to be very faint, measures of position angles were principally attended to in preference to those of distances, which however it was in the case of Deimos found quite practicable to take. The Position Angles observed in 1879 indicate corrections to Professor Hall's periods of revolution of Deimos and Phobos equal to respectively $+ 0^{s}{\cdot}56$ and $- 1^{s}{\cdot}15$, while the Distances of Deimos from 1879 produce a correction to Prof. Hall's value of the Mean Distance $= + 0''{\cdot}20$. The remainder of the volume contains observations of minor planets and comets and a list of corrections to Bond's Zones furnished by Prof Peters.

Statement of Work done at the Harvard College Observatory during the years 1877–1882. By E. C. Pickering (Cambridge, 1882, 23 pp. 8vo.) In the Autumn of 1878 about seventy ladies and gentlemen of Boston and vicinity subscribed an annual sum of five thousand dollars for five years to enable the Director of Harvard College Observatory to keep three principal instruments (the 15-in. Refractor, the Meridian Circle and the Meridian Photometer) in constant use; and to get a large number of old observations reduced and printed. As the five years are now about to expire, a subscription of one hundred thousand dollars has been opened, and considering the well-known liberality of the American people where science is concerned, there can be little doubt that this sum will speedily be subscribed. The present report contains a short summary of the work done since the appointment of the present Director in February 1877, and concludes with a list of publications of all the employés of the Observatory.

A Plan for securing Observations of Variable Stars. By E. C. Pickering (Cambridge, 1882, 15 pp. 8vo.) This pamphlet is chiefly addressed to amateur astronomers, and it is very much to be hoped that the invitation of Prof. Pickering will be largely responded to. After enumerating the five classes into which Variables may be divided (the types being T Coronæ, *o* Ceti, *α* Orionis, *β* Lyræ and *β* Persei), the chances of discovering such objects by a certain number of observations are next discussed, after which the problems to be undertaken are defined as follows :—

> 1. To observe all the long period variables once or twice every month, throughout their variations, according to such a system that all the observations may be reduced to the same absolute scale of magnitudes.
>
> 2. To observe the stars whose variability is suspected and prove either that they are really variable or that they do not belong to the first, second or fourth-class. If any are thought to belong to the fifth class (Algol), to watch them until such variation is proved or shown to be improbable.

This is to be done by Argelander's method of comparing a variable star with an adjacent star of nearly equal brightness, expressing the difference in grades, so that the interval is equal to one grade if one sometimes appears brighter and sometimes fainter, but more frequently brighter than the other ; to two grades if one is certainly brighter than the other, the difference being very small, and so on. The experience of a great many years has shown this method to be capable of furnishing results of great accuracy. It is found in practice that a grade thus estimated will slightly exceed a tenth of a magnitude. Professor Pickering offers to furnish intending observers with copies of the pamphlet and any necessary information, and it is proposed that the observations should be sent to him once a month.

The Solar Parallax from Meridian Observations of Mars in 1877. By J. R. Eastman (Washington Observations for 1877, Appendix III., 43 pp. 4to). The Washington Observations as compared with those at Melbourne, Sydney and the Cape gave the Parallax equal to respectively 8″·97, 8″·89, 8″·90, while Melbourne and Leiden gave 8″·97 and Melbourne and Harvard College 9″·14. The last result is supposed to be influenced by the method of observing over inclined threads at Harvard ; omitting it the Author adopts the final result 8″·953. It has been suggested by Mr. Gill that the large parallax found from Meridian Observations of Mars may arise from the chromatic dispersion of the atmosphere which fringes the upper limb of the planet with blue and the lower with red, inclining the observer to cut more deeply into the blue or violet limb than into the more visible red limb. Experiments on Jupiter with illuminations of various colours have however not revealed to Prof. Eastman any such tendency.

UEBER DAS KEPLER'SCHE PROBLEM.

VON PROFESSOR J. C. KAPTEYN.

In diesem Aufsatze wird eine Reihe entwickelt für die Lösung des Kepler'schen Problems, die für alle Planetenbahnen, auch die am meisten excentrischen, ausserordentlich convergent ist. Diese Convergenz ist so gross, dass eine *directe* Berechnung der excentrischen Anomalie, mit Zuhülfenahme einer mässig grossen Tafel eben so bequem, oder sogar noch etwas bequemer wird, als nach den gebraüchlichen Näherungsverfahren. Aber auch abgesehen von der Frage, in wiefern diese Lösung für den Praxis zu empfehlen ist, möchte die Reihe vielleicht nicht ohne Interesse sein.

Es sei M die mittlere, E die excentrische Anomalie, e die Excentricität, $\Delta \mathrm{E} = \mathrm{E} - \mathrm{M}$,

$$\mathrm{R} = \frac{e \sin \mathrm{M}}{1 - e \cos \mathrm{M}}, \quad \mathrm{T} = \frac{e \cos \mathrm{M}}{1 - e \cos \mathrm{M}}.$$

Die erwähnte Reihe wird dann erhalten, wenn man E − M entwickelt nach den steigenden Potenzen entweder von cotang M oder von T. Das Ergebniss der ersten Entwickelungsart wird man in die folgende Form bringen können—

$$\begin{aligned}(1)\ \mathrm{E} - \mathrm{M} = \alpha + \operatorname{cotg} \mathrm{M} & \left[-\tfrac{1}{6}\alpha^4 + \frac{11}{120}\alpha^6 - \frac{337}{5040}\alpha^8 + \frac{16711}{362880}\alpha^{10} - \frac{1279301}{39916800}\alpha^{12} + \frac{138623707}{6227020800}\alpha^{14} - \ldots \right] \\ + \cot^2 \mathrm{M} & \left[+ \frac{1}{12}\alpha^7 - \frac{13}{120}\alpha^9 + \frac{7517}{60480}\alpha^{11} - \frac{228199}{1814400}\alpha^{13} + \frac{9528949}{79833600}\alpha^{15} - \ldots \right] \\ + \cot^3 \mathrm{M} & \left[- \frac{1}{18}\alpha^{10} + \frac{101}{864}\alpha^{12} - \frac{166549}{907200}\alpha^{14} + \ldots \right] \\ + \cot^4 \mathrm{M} & \left[+ \frac{55}{1296}\alpha^{13} - \frac{403}{3240}\alpha^{15} + \ldots \right] \\ + & \ldots\ldots\ldots\ldots\ldots\ldots\end{aligned}$$

wo—

$$(2) \qquad \alpha = \mathrm{R} \cos \alpha$$

Es ist daher α immer kleiner (in absoluten Werth) als $\frac{e \sin \mathrm{M}}{1 - e \cos \mathrm{M}}$.

In eine Reihe entwickelt ergiebt sich für α—

$$(3) \qquad \alpha = \mathrm{R} - \tfrac{1}{2}\mathrm{R}^3 + \frac{13}{24}\mathrm{R}^5 - \frac{541}{720}\mathrm{R}^7 + \frac{9509}{8064}\mathrm{R}^9 - \frac{7231801}{3628800}\mathrm{R}^{11} + \ldots$$

Wird dieser Werth für α in (1) eingeführt, so ist die erhaltene Reihe in Wirklichkeit nicht verschieden von einer Reihe, die schon von Keill

(Introductio ad veram Astronomiam) entwickelt wurde, und die später noch häufig besprochen worden ist. Die Glieder der Keill'schen Reihe aber, welche nach den aufsteigenden Potenzen von e sin M geordnet sind, werden bald sehr verwickelt, und dies ist wohl die Ursache, warum man nirgends (wenigstens so weit mir bekannt ist) eine grössere Zahl von Gliedern entwickelt findet. Wäre dies wohl geschehen, wären z. B. die Glieder bis zur zehnten oder funfzehnten Ordnung berechnet, so hätte man wahrscheinlich schon längst bemerkt, dass diese Reihe sich in eine Form wie die obige bringen liesse. Wie dem auch sei, für eine *directe* Berechnung der excentrischen Anomalie bei einigermassen grossen Excentricitäten ist die Keill'sche Reihe, wie sie bisher gegeben wurde, gewiss nicht geeignet.

Nach der Reihe (1) wird dagegen die directe Rechnung sehr bequem ausfallen, denn ihre grosse Convergenz und bequeme Form sind augenscheinlich. Jedes Glied ist (wenn e als eine kleine Grösse erster Ordnung betrachtet wird) um drei Ordnungen kleiner als das unmittelbar vorhergehende. Das Fremdartige einer Entwickelung nach einer Grösse, die unendlich gross werden kann, verschwindet sofort durch die Bemerkung, dass nach (3) die Coefficienten alle R, also auch sin M enthalten, und zwar mit einem Exponenten, der immer viel grösser ist als die von cot M. Es wird dieses noch klarer, wenn man T statt cot M einführt mittelst—

$$\mathrm{T} = \frac{e \cos \mathrm{M}}{1 - e \cos \mathrm{M}} = \mathrm{R} \cot \mathrm{M} = \frac{\alpha}{\cos \alpha} \cot \mathrm{M}$$

Man erhält sofort—

$$\begin{aligned}(4) \qquad \mathrm{E} - \mathrm{M} = \alpha &+ \mathrm{T} \cos \alpha \left[- \frac{1}{6} \alpha^3 + \frac{11}{120} \alpha^5 - \frac{337}{5040} \alpha^7 + \;.\;.\;.\;.\;. \right] \\ &+ \mathrm{T}^2 \cos^2 \alpha \left[+ \frac{1}{12} \alpha^5 - \frac{13}{120} \alpha^7 + \;.\;.\;.\;.\;. \right] \\ &+ \mathrm{T}^3 \cos^3 \alpha \left[- \frac{1}{18} \alpha^7 + \;.\;.\;.\;.\;. \right] \\ &+ \;.\;.\;.\;.\;.\;.\;.\;.\;.\end{aligned}$$

wo keine Grössen mehr vorkommen, die unendlich werden können. Es scheint mir aber, dass die Formel (1) für die Berechnung ein wenig bequemer sein wird und ich werde daher bei dieser Reihe stehen bleiben. Es sei kurz—

$$(5) \qquad \mathrm{E} - \mathrm{M} = \alpha + \beta \cot \mathrm{M} + \gamma \cot^2 \mathrm{M} + \delta \cot^3 \mathrm{M} + \varepsilon \cot^4 \mathrm{M} + \zeta \cot^5 \mathrm{M} + \;.\;.\;.\;.$$

Die Coefficienten α, β, γ . . . . sind alle reine Functionen von R und können also mit dem Argumente R in eine Tafel gebracht werden. Mit dieser Tafel wird dann die Berechnung von E sehr einfach. Zur Erleichterung der Uebersicht über die Zahl der noch in Betracht kommenden Glieder und ihrer Grösse, berechnete ich das hier folgende kleine Tableau, worin der beiläufige Werth des Maximums der Glieder für verschiedene Werthe von e angegeben ist (dieses Tableau ist nur roh berechnet)—

MAXIMAL-WERTHE.

e	R	R — α *	$\beta \cot M$	$\gamma \cot^2 M$	$\delta \cot^3 M$	$\epsilon \cot^4 M$	$\zeta \cot^5 M$
0·1	5°·75	1′·8	1″·35	0″·000			
0·2	11·7	14	26	0·055	0″·000		
0·3	18·0	48	156	1·3	0·014	0″·000	
0·4	25·0	2° 0	590	13·5	0·42	0·015	0″·001
0·5	33·1	4 9	1730	90·6	6·30	0·50	0·043

Es wird sonach schon das Glied $\epsilon \cot^4 M$ für *alle* bekannte Planetenbahnen immer unter 0″·01 bleiben, denn selbst für die Bahn der Aethra (die am meisten excentrische von allen) wo die Excentricität = 0·383 ist, erreicht dieses Glied im Maximo nur den Werth 0″·007. Sogar das vorige Glied kann nur für sehr wenige Planeten einen merklichen Einfluss haben und zwar nur innerhalb gewisser Grenzen der Anomalie. Wie leicht übrigens die Berechnung der noch übrigbleibenden Glieder ausfallen wird, sieht man sofort ein.

Einige Beispiele werden dieses noch näher beleuchten. Zu dem Ende wurden die folgenden Bruchstücke einer allgemeinen Tafel berechnet: dabei wurde die geschlossene Form der Coefficienten benutzt, die weiter unten gegeben wird. Die Vorzeichen, die den Logarithmen beigesetzt sind, beziehen sich natürlich auf den Zahlen, zu welchen diese Logarithmen gehören†—

R	α — R	log β	log γ	log δ	log ϵ	log ζ
± 8° 17′	∓ 5′ 4″·77	— 1·1539	± 8·31			
18	6·59	1·1573	8·32			
± 20 0	∓ 1° 4 49·80	— 2·58661	± 0·8084	— 9·15		
1	58·81	2·58787	0·8106	9·16		
± 32 0	∓ 3 47 55·13	— 3·252757	± 1·95832	— 0·7842	± 9·667	— 8·58
1	48 13·30	3·253434	1·95948	0·7859	9·669	8·59

Diese letzten Werthe kommen nur für Kometen in Betracht. Für Planeten wird R nie viel grösser als 20° (für Aethra im Maximo 23°·8). Es sei nun E zu berechnen in den drei folgenden Beispielen—

$$\left.\begin{aligned}\log e &= 9{\cdot}3897262\\ M &= 332^\circ\ 28'\ 54''{\cdot}77\end{aligned}\right\}\ \text{Juno, Theoria Motus, Art. 13.}$$

$$\left.\begin{aligned}\log e &= 9{\cdot}5833466\\ M &= 40^\circ\ 7'\ 20''{\cdot}00\end{aligned}\right\}\ \text{(132) Aethra.}$$

$$\left.\begin{aligned}\log e &= 9{\cdot}7395859\\ M &= 33^\circ\ 27'\ 50''{\cdot}00\end{aligned}\right\}\ \text{Komet Faye—Möller.}$$

Die Berechnung von R nach $R = \frac{e \sin M}{1 - e \cos M}$ ergiebt—

1^{es} Beispiel — 8° 17′ 59″·33
2^{es} „ + 20 0 28·17
3^{es} „ + 32 0 11·05

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* Zur Erleichterung der Interpolation wird es den Vorzug verdienen R — α statt α in eine Tafel zu bringen.

† Wer sich durch noch andere Beispiele von der Bequemheit der Methode überzeugen will, kann natürlich schon mit diesen Bruchstücken eine grosse Menge von Problemen auflösen.

Weiter stellt sich für das erste Beispiel die ganze Rechnung so: mit dem

Argumente R findet man in der Tafel	+ 5′ 6″ 57	− 1·1573	− 8·31
ferner ist log cot M, $\cot^2 M$		− 0·2832	+ 0·57
		+ 1·4405	− 8·88

so darf sich mit nur zweimaligem Eingehen in eine 4 stellige Logarithmentafel ergeben—

M =	332° 28′ 54″·77	
R		− 8° 17′ 59″·33
$\alpha - R$	+ 5′ 6·57	
$\beta \cot M$	+ 27·57	
$\gamma \cot^2 M$		− 08
	332° 34′ 28″·91	
	− 8 17 59·41	

E = 324 16 29·50 was vollkommen mit Gauss's Berechnung stimmt. Fast eben so schnell findet man für das zweite Beispiel—

M	40° 7′ 20″·00	
R	20 0 28·17	
$R - \alpha$		− 1° 4′ 54″·03
$\beta \cot M$		− 7 38·68
$\gamma \cot^2 M$	9·08	
$\delta \cot^3 M$		− 0·23
	60° 7′ 57″·25	
	− 1 12 32·94	

E = 58 55 24·31 richtig innerhalb 0″·01.

Im letzten Beispiele endlich wird gefunden—

M	+ 33° 27′ 50″·00	
R	32 0 11·05	
$R - \alpha$		− 3° 47′ 58″·48
$\beta \cot M$		45 8·28
$\gamma \cot^2 M$	3 28·04	
$\delta \cot^3 M$		21·08
$\varepsilon \cot^4 M$	2·43	
$\zeta \cot^5 M$		0·30
$\eta \cot^6 M$	0·04	
	65 31 31·56	
	− 4 33 28·14	

E = 60 58 3·42

richtig innerhalb 0″·03; dieser Fehler ist fast ganz den unvermeidlichen Fehlern der siebenstelligen Berechnung von R zuzuschreiben.

Nachdem jetzt, wie ich hoffe, die Bequemlichkeit der Berechnung dargethan ist, gehe ich über zu der Entwicklung der Reihe selbst. Diese ist äusserst leicht und für die Reihe nach Potenzen von T fast identisch mit der der Reihe nach cot M. Ich werde mich daher auf die zweite Form beschränken, weil diese hier in Anwendung kam. Wie schon oben gezeigt wurde, ergiebt sich aus diesen letzteren dann unmittelbar die Reihe nach Potenzen von T.

Die Gleichung $M = E - e \sin E$ kann man auch so schreiben:

$$\Delta E = e \sin (M + \Delta E) = e \sin M \cos \Delta E + e \cos M \sin \Delta E \tag{6}$$

welche, wenn man einführt—

$$R = \frac{e \sin M}{1 - e \cos M} \qquad S = \cot M \tag{7}$$

in die folgende ungewandelt wird—

$$-R \cos \Delta E + \Delta E = RS (\sin \Delta E - \Delta E). \tag{8}$$

Man setze nun—

$$\Delta E = \alpha + \beta S + \gamma S^2 + \delta S^3 + \varepsilon S^4 + \zeta S^5 + \ldots\ldots \tag{9}$$

oder kurz—

$$\Delta E = \alpha + \tau \tag{10}$$

so dass man hat—

$$\tau = \beta S + \gamma S^2 + \delta S^3 + \varepsilon S^4 + \zeta S^5 + \ldots\ldots \tag{11}$$

Entwickelt man jetzt $\sin \tau$ und $\cos \tau$ nach steigenden Potenzen von S (mittelst der Gleichungen $\sin \tau = \tau - \frac{1}{6}\tau^3 + \ldots$, $\cos \tau = 1 - \frac{1}{2}\tau^2 + \ldots$) und setzt man diese ein in—

$$\sin \Delta E = \sin \alpha \cos \tau + \cos \alpha \sin \tau \tag{12}$$

$$\cos \Delta E = \cos \alpha \cos \tau - \sin \alpha \sin \tau \tag{13}$$

so ergeben sich sofort—

$$\begin{aligned} \sin \Delta E = \sin \alpha &+ S \cdot \beta \cos \alpha + S^2 [\gamma \cos \alpha - \tfrac{1}{2} \beta^2 \sin \alpha] \\ &+ S^3 \left[\delta \cos \alpha - \frac{1}{2 \cdot 3} \beta^3 \cos \alpha - \beta\gamma \sin \alpha\right] \\ &+ S^4 \left[\varepsilon \cos \alpha - \frac{1}{2} \beta^2\gamma \cos \alpha - \left(\beta\delta + \frac{1}{2} \gamma^2 - \frac{1}{2 \cdot 3 \cdot 4} \beta^4\right) \sin \alpha\right] \\ &+ \ldots \end{aligned} \tag{14}$$

$$\begin{aligned} \cos \Delta E = \cos \alpha &+ S \, [-\beta \sin \alpha] + S^2 [-\gamma \sin \alpha - \tfrac{1}{2} \beta^2 \cos \alpha] \\ &+ S^3 \left[-\delta \sin \alpha + \frac{1}{2 \cdot 3} \beta^3 \sin \alpha - \beta\gamma \cos \alpha\right] \\ &+ S^4 \left[-\varepsilon \sin \alpha + \frac{1}{2} \beta^2\gamma \sin \alpha - \left(\beta\delta + \frac{1}{2} \gamma^2 - \frac{1}{2 \cdot 3 \cdot 4} \beta^4\right) \cos \alpha\right] \\ &+ \Big[-\zeta \sin \alpha + \left(\frac{1}{2} \beta^2\delta + \frac{1}{2} \beta\gamma^2 - \frac{1}{2 \cdot 3 \cdot 4 \cdot 5} \beta^5\right) \sin \alpha \\ &\qquad - \left(\beta\varepsilon + \gamma\delta - \frac{1}{2 \cdot 3} \beta^3\gamma\right) \cos \alpha\Big] \\ &+ \ldots \end{aligned} \tag{15}$$

Werden jetzt (9), (14), (15) in (8) eingeführt und die Coefficienten der Potenzen von S jeder für sich = 0 gesetzt, so erhalten wir die Gleichungen—

$$
(16) \begin{cases}
a - R\cos a = 0 \\
\beta\,(R\sin a + 1) = R\;(\sin a - a) \\
\gamma\,(R\sin a + 1) + \frac{1}{2}\beta^2 R\cos a = R\beta\,(\cos a - 1) \\
\delta\,(R\sin a + 1) - \frac{1}{2\cdot 3}\,\beta^3 R\,\sin\,a + \beta\gamma R\,\cos\,a = R\gamma\,(\cos a - 1) - \frac{1}{2}\,\beta^2 R\sin a \\
\varepsilon\,(R\sin a + 1) - \frac{1}{2}\beta^2\gamma R\,\sin\,a + \left(\beta\delta + \frac{1}{2}\gamma^2 - \frac{1}{2\cdot 3\cdot 4}\beta^4\right) R\cos a \\
\qquad = R\delta\,(\cos a - 1) - \frac{1}{2\cdot 3}\beta^3 R\cos a - \beta\gamma R\sin a \\
\zeta\,(R\sin a + 1) - \left(\frac{1}{2}\beta^2\delta + \frac{1}{2}\beta\gamma^2 - \frac{1}{2\cdot 3\cdot 4\cdot 5}\,\beta^5\right) R\sin a + \left(\beta\varepsilon + \gamma\delta - \frac{1}{2\cdot 3}\,\beta^3\gamma\right) R\cos a \\
\qquad = R\varepsilon\,(\cos\,a - 1) - \frac{1}{2}\beta^2\gamma R\,\cos\,a - \left(\beta\delta + \frac{1}{2}\,\gamma^2 - \frac{1}{2\cdot 3\cdot 4}\,\beta^4\right) R\sin a
\end{cases}
$$

u.s.w.

durch welche die Coefficienten von (9) bestimmt werden. Werden diese Coefficienten β, γ, δ . . . . hieraus nach steigenden Potenzen von a entwickelt, so ergeben sich die Reihen, die oben (in der Gleichung (1)) für diese Grössen angesetzt sind. Man sieht aber auch schon unmittelbar aus den Formeln (16), dass wenn e, also auch R, als eine kleine Grösse erster Ordnung betrachtet wird—

a	eine Grösse	1er	Ordnung sein wird	
β	,,	4er	,,	
γ	,,	7er	,,	
δ	,,	10er	,,	u.s.w.

Es ist leicht zu beweisen, dass ganz allgemein die Ordnung eines Coefficienten um drei Ordnungen höher ist als die des unmittelbar vorhergehenden. Dazu führe ich statt a, β, γ, δ . . . . die Bezeichnungen a, β_1, β_2, β_3 . . . . ein; es ist demnach—

$$(17) \qquad \Delta E = a + \tau$$

$$(18) \qquad \tau = \beta_1 S + \beta_2 S^2 + \beta_3 S^3 + \ldots\ldots\ldots$$

und ich werde nun zeigen, dass wenn bis β_m inclusive die Ordnung der Coefficienten β_n $3n + 1$ ist, dann auch noch β_{m+1} von der Ordnung $3\,(m + 1) + 1 = 3m + 4$ sein wird. Es bedarf keines näheren Nachweises dass, sobald es gelungen ist dieses zu zeigen, damit auch der gewünschte Beweis geliefert ist.

Bei den hier folgenden Reihen wird man jedesmal die Ordnung des allgemeinen Gliedes angegeben finden. Diese Ordnung wird aber im Voraus nur als gültig anzusehen sein bis auf das letzte noch angesetzte Glied inclusive. Es wird also die Bezeichnung—

$$(19) \qquad \tau = \beta_1 S + \beta_2 S^2 + \ldots + \beta_m S^m + \ldots \quad (\beta_n \text{ Ordnung } 3n + 1)$$

diese Bedeutung haben: bis auf β_m inclusive ist die Ordnung der Coefficienten β_n $3n + 1$.

Entwickelt man die Potenzen von τ so kommt zuerst—

$$\tau^2 = \beta_1{}^2 S^2 + \ldots + [\beta_1\beta_m + \beta_2\beta_{m-1} + \ldots + \beta_{m-1}\beta_2 + \beta_m\beta_1] S^{m+1} + \ldots$$

Es wurde aber vorausgesetzt, dass β_1 von der vierten, β_2 von der siebenten Ordnung, β_m von der $(3m + 1)$ten Ordnung ist, folglich ist, wenn die Coefficienten mit γ_n bezeichnet werden—

$$(20) \qquad \tau^2 = \gamma_2 S^2 + \gamma_3 S^3 + \ldots + \gamma_{m+1} S^{m+1} + \ldots \quad (\gamma_n \text{ Ordnung } 3n + 2).$$

Ganz ebenso wird gefunden—

$$(21) \qquad \tau^3 = \delta_3 S^3 + \delta_4 S^4 + \ldots + \delta_{m+2} S^{m+2} + \ldots \quad (\delta_n \text{ Ordnung } 3n + 3).$$

$$(22) \qquad \tau^4 = \epsilon_4 S^4 + \epsilon_5 S^5 + \ldots + \epsilon_{m+3} S^{m+2} + \ldots \quad (\epsilon_n \text{ Ordnung } 3n + 4).$$

Werden mit Hülfe dieser Formeln und der Gleichungen—

$$\cos\tau = 1 - \tfrac{1}{2}\tau^2 + \frac{1}{2\cdot3\cdot4}\tau^4 \ldots\ldots$$

$$\sin\tau = \tau - \frac{1}{2\cdot3}\tau^3 + \frac{1}{2\cdot3\cdot4\cdot5}\tau^5 \ldots\ldots$$

die Werthe von cos τ und sin τ nach steigenden Potenzen von S entwickelt so ergiebt sich leicht—

$$(23) \left\{\begin{array}{lll} \cos\tau = 1 + A_2 S^2 + A_3 S^3 + \ldots\ldots & + A_{m+1} S^{m+1} + \ldots & (A_n \text{ Ordnung } 3n + 2) \\ \sin\tau = B_1 S + B_2 S^2 + \ldots\ldots & + B_m S^m + \ldots & (B_n \text{ Ordnung } 3n + 1). \\ \sin\tau - \tau = C_3 S^3 + C_4 S^4 + \ldots\ldots & + C_{m+2} S^{m+2} + \ldots & (C_n \text{ Ordnung } 3n + 3). \end{array}\right.$$

Es genügt aber ΔE der Gleichung (8), die hier mit Zuhülfenahme von (10) geschrieben wird—

$$\Delta E = R\cos\alpha\cos\tau - R\sin\alpha\sin\tau + RS\sin\alpha\cos\tau + RS\cos\alpha\sin\tau - RS\alpha - RS\tau$$

oder—

$$(24) \ \Delta E = R\cos\alpha\cos\tau - R\sin\alpha\sin\tau + RS\sin\alpha\cos\tau + RS(\sin\tau - \tau) + RS\sin\tau(\cos\alpha - 1) - R\alpha S$$

R und α sind beide erster Ordnung. Wird also durch Einführung der Formeln (23) das zweite Glied dieser Gleichung entwickelt nach steigenden Potenzen von S, so wird man bemerken dass in der Entwicklung aller Glieder, mit alleiniger Ausnahme von der des zweiten Gliedes, die Coefficienten von S^{m+1} von der Ordnung $3m + 4$ oder höher sind. Allein auch dieses Glied wird in die gewünschte Form gebracht, wenn man die Identität—

$$R\sin\alpha\Delta E = R\alpha\sin\alpha + R\tau\sin\alpha$$

addirt, wodurch die Form erhalten wird—

$$(25) \quad \Delta E(1 + R\sin\alpha) = R\alpha\sin\alpha - R\alpha S + R\cos\alpha\cos\tau - R\sin\alpha(\sin\tau - \tau) \\ + RS\sin\alpha\cos\tau + RS(\sin\tau - \tau) + RS\sin\tau(\cos\alpha - 1).$$

Setzt man hierin die Werthe (23) ein, so wird die Ordnung der Coefficienten von S^{m+1} sein—

im 3^{en} Gliede $3(m+1)+3=3m+6$
„ 4^{en} „ $3(m+1)+5=3m+8$
„ 5^{en} „ $3m+4$
„ 6^{en} „ $3m+4$
„ 7^{en} „ $3m+4$

Es ist also der Coefficient von S^{m+1} in der Entwickelung von $\Delta E\,(1+R\sin\alpha)$ von der Ordnung $3m+4$; die nämliche Ordnung wird dieser Coefficient also auch in der Entwickelung von ΔE nach steigenden Potenzen von S haben.

Zum Schluss erlaube ich mir noch ein Paar Bemerkungen hinzufügen über die Construction der Tafeln, die für die hier gegebene Lösung des Kepler'schen Problems erforderlich sind. Die Berechnung dieser Tafeln ist nicht schwierig, mag man dazu (wenigstens für kleinere R) die Reihenentwicklungen der Coefficienten verwenden, die in der Gleichung (1) gegeben sind oder mit den geschlossenen Formen (9) rechnen. Letzteres verdient wohl im Allgemeinen den Vorzug. Die Lösung der in beiden Fällen auftretenden transcendenten Gleichung $\alpha=R\cos\alpha$ (die nur einen besondern Fall der Kepler'sche Aufgabe bildet) wird nicht die geringste Schwierigkeit darbieten. Wenn α_1 eine erste Annäherung ist (wenn man keine bessere hat kann dafür R selbst genommen werden) so ist—

$$(26)\qquad \alpha=\alpha_1+\frac{R\cos\alpha_1-\alpha_1}{1+R\sin\alpha_1}=R\cos\alpha_1-\frac{R\sin\alpha_1}{1+R\sin\alpha_1}(R\cos\alpha_1-\alpha_1)$$

d. h. α wird, wenn der Fehler der Näherung eine Grösse dritter Ordnung ist (was schon bei der Näherung $\alpha_1=R$ der Fall ist) nur um eine Grösse fünfter Ordnung von $R\cos\alpha_1$ verschieden sein. Man wird also sehr leicht erst eine kleine Tafel etwa von Grad zu Grad berechnen können (mit 4 oder 5 stelligen Logarithmen) worin genäherte Werthe für α_1 und $\frac{R\sin\alpha_1}{1+R\sin\alpha_1}=P$ angegeben werden. Mit Hülfe einer solchen kleinen Tafel wird dann die Berechnung einer ausführlichen Tafel für α, selbst wenn diese zehnstellig geführt wird, fast nie mehr als eine directe Rechnung nach—

$$(27)\qquad \alpha=R\cos\alpha_1-P(R\cos\alpha_1-\alpha_1)$$

erfordern.

Was nun den Umfang der erforderlichen Tafel betrifft, so geht aus den hier gegebenen Bruchstücken hervor, dass ein Intervall von 1′ in den Argumenten ausreicht. Wird nun ein Tafel berechnet *nur* für Planetenbahnen (es scheint mir zweckmässig für die weniger excentrischen Kometenbahnen eine besondere Tafel zu besitzen) so bleibt R unter 24° und die Zahl der Argumente also unter 1440. Die Grösse R hat man in Bogensekunden zu berchnen, weshalb es vielleicht vortheilhafter ist das Argument R in der Tafel in Sekunden ausgedrückt zu geben, und bei dieser Einrichtung würde ein Intervall von 100″ vorzuziehen sein, wodurch die Zahl der Argumente auf 864 herabsinken würde. Eine solche Tafel wird also auf keinen Fall eine sehr grosse Ausdehnung zu haben brauchen.

Nachdem das obige geschrieben wurde, kam mir die neue Auflage des vorzüglichen Lehrbuchs zur Bahnbestimmung von Herrn Oppolzer zu Gesicht. Daselbst wird ein Näherungsverfahren benutzt und auf ein Beipsiel angewendet, das mir eine sehr erwünschte Gelegenheit anbietet die Bequemlichkeit der hier vorgetragenen Methode mit einer der besten Näherungsmethoden zu vergleichen. Denn das dort gegebene "Encke'sche durch Herz wesentlich erweiterte Verfahren" stützt sich gerade auf eine Modification der Keill'schen, also auch der hier gegebenen Reihe; davon wurden aber die Glieder siebenter Ordnung "die schon etwas mehr zusammengesetzt sind" weggelassen.

Ich werde um diese Vergleichung sofort ganz übersichtlich zu machen, die Coefficienten der Reihe (1) angeben mit dem Argument das bei Oppolzer S. 55 mit $\alpha\eta$ bezeichnet wird. Die Berechnung des Arguments wird dadurch zwar ein wenig länger, dagegen wird die Hülfstafel noch ein wenig einfacher, so dass man die Werthe der Coefficienten fast auf den ersten Blick der Tafel entnehmen kann.

Die Berechnung wird von Herrn Oppolzer geführt nach den Formeln (die Buchstaben α, β, γ, δ sind hier, um Verwirrung vor zu beugen mit Accente versehen)—

$$(A) \quad \left\{ \begin{aligned} \operatorname{tg} y &= \frac{e \sin M}{1 - e \cos M} \\ \eta &= \operatorname{tg} y \cos y \\ x'' &= \alpha'\eta + \beta' \cot M\eta^4 + \gamma'\eta^5 + \delta' \cot M \,.\, \eta^6 \\ \text{wo } \log \alpha' &= 5{\cdot}3144251 \qquad \log \beta = 4{\cdot}5363_n \\ \log \gamma' &= 4{\cdot}5363 \qquad\qquad \log \delta = 4{\cdot}2766 \end{aligned} \right.$$

Nach der Formel (1) wird man, wenn man das Argument $\alpha'\eta$ gebrauchen will, zu rechnen haben nach ($\alpha - \alpha'\eta = A$ gesetzt)—

$$(B) \quad \left\{ \begin{aligned} \operatorname{tg} y &= \frac{e \sin M}{1 - e \cos M} \\ \eta &= \operatorname{tg} y \cos y \\ x'' &= \alpha'\eta + A + \beta \cot M + \gamma \cot^2 M \end{aligned} \right.$$

wo A, log β und log γ aus einer kleinen Tafel mit dem Argument $\alpha'\eta$ zu nehmen sind. Nach diesen Formeln mag nun das oben behandelte erste Beispiel, das auch Oppolzer gewählt hat, berechnet werden. Es wird sich dabei zeigen, dass aus der Tafel das folgende Stück erforderlich ist—

$\alpha\eta$	A	log β''	log γ''
± 8° 12′	± 2″·06	— 1·1543	± 8·31
13′	2·08	1·1578	8·32

wo die Vorzeichen, die den Logarithmen beigesetzt sind, sich wieder auf den dazu gehörigen Zahlen beziehen. Man sieht, dass man wirklich fast auf den ersten Blick die Werthe von A, log β'', log $\gamma,''$ der Tafel entnehmen kann. Ich setze nun die beiden Rechnungen einander gegenüber, die des Herrn Oppolzer so, wie man sie S. 56 seines Lehrbuchs findet, die nach den Formeln (B) genau eben so vollständig (bei Oppolzer sind die α', β', γ', δ', die eins für allemal auf einem Streifen Papier geschrieben werden, weggelassen; bei meiner Rechnung ist dies nur mit α' den Fall). Ein Versehen in der Oppolzer'schen Berechnung, wodurch er zu dem unrichtigen Schlusse geführt wurde, dass schon die erste Näherung das richtige Resultat ergiebt, ist hier verbessert—

Nach den Formeln A (Oppolzer's Näherung).		Nach den Formeln B (direct).	
sin M	9·664 6693_n	sin M	9·664 6693_n
cos M	9·947 8574	cos M	9·947 8574
e cos M	9·337 5836	e cos M	9·337 5836
1 : (1 − e cos M)	0·106 5502	1 : (1 − e cos M)	0·106 5502
e sin M	9·054 3955_n	e sin M	9·054 3955^n
tg y	9·160 9457_n	tg y	9·160 9457_n
cos y	9·995 4905	cos y	9·995 4905
η	9·156 4362_n	η	9·156 4362^n
log $\alpha'\eta$	4·470 8613_n	log $\alpha'\eta$	4·470 8613_n

So weit sind die beiden Berechnungen identisch.

cot M	$0·2832_n$		$\alpha'\eta$ −	8° 12′ 50″·68
β' cot M	4·8195	Tafel	A −	2·08
η^4	6·6257		log β	$1·1573_n$
			log γ	$8·32_n$
η^5	$5·7822_n$		cot M	$0·2832_n$
			cot² M	0·57
δ' cot M	$4·5598_n$			
η^6	4·9386			

$\alpha'\eta$ −	8° 12′ 50″·68	$\alpha'\eta$ + A −	8° 12′ 52″·76
β' cot M η^4 +	27·87	β cot M	+ 27·57
$\gamma\eta^5$ −	2·08	γ cot² M	− 0·08
δ' cot M η^6 −	0·32		
x'' = −	8° 12′ 25″·21	x'' = −	8° 12′ 25″·27

Wie man sieht, ist die Arbeit in den beiden Fällen kaum verschieden. Die Zahl der Operationen die im Kopfe oder auf dem Papiere auszuführen sind, ist für die Formeln (B) noch etwas geringer, aber dabei wird eine *besondere* Tafel erfordert, was bei der Berechnung nach den Formeln (A) nicht der Fall ist. *Dagegen* aber erlangt man nach dem Oppolzer'schen Verfahren auch nur eine *Näherung,* die zwar immer nur wenig von der Wahrheit entfernt ist (in den extremsten Fällen unseres Planetensystems nur wenige Sekunden), dennoch eine weitere Rechnung nach den Formeln—

$$M_1 = E_1 - e'' \sin E_1$$
$$E_2 = E_1 + \frac{M - M_1}{1 - e \cos E_1}$$

nothwendig macht, indem nach den Formeln (2) die Rechnung immer den Werth von E sofort richtig giebt.

Durch diese Vergleichung wird also, wie ich glaube, die am Anfange dieses Aufsatzes aufgestellte Behauptung genügend gerechtfertigt.

Gröningen, Holland,
December 1882.

RECENT LITERATURE.

Resultados del Observatorio Nacional Argentino en Córdoba, B. A. Gould. Vol. II., Observaciones del Año 1872 (Buenos Aires 1882, LXXVIII. and 296 pp. 4to). The greater part of this splendid volume is taken up by the first instalment of the zone-observations which were commenced at Córdoba on September 9, 1872, and closed on Aug. 9, 1875, during which time about 105000 single observations were made. Of these the present volume contains about 13000. The zones comprise the part of the heavens between 23° and 80° South Declination, and as a security against constant errors the plan included the formation of a more accurate catalogue, containing a number of Stars from each zone observed three or four times each with all possible care. The meridian circle was constructed by Repsold, it has a telescope of 122^{mm} and a circle of 716^{mm} diameter, graduated to 4′. All the constants of the instrument have been investigated and the results are all given in the introduction. The right ascensions of all the stars observed at Córdoba depend upon those of the U.S. Coast Survey Catalogue of Fundamental Stars (2nd Ed. 1866) with a few slight modifications, the declinations are deduced from nadir observations, the latitude being assumed equal to $-31° \; 25' \; 15''\cdot 0$. The zones were 2° in width as far as 47° Decl., thence increasing gradually with the declination; they were generally one hundred minutes long. The transits were always observed by Dr. Gould, generally over three wires, and were registered on a chronograph, while an assistant read off one miscroscope, which was compared with the other three at the beginning and end of the zone. 410 Catalogue and Time Stars were observed in 1872 and the separate and mean results for 1875·0 are given. To this epoch the zone Stars are also reduced, and as there is an index to the Zones observed in 1872 at the end of the volume it is very simple to find any star required.

Celestial Charts made at the Litchfield Observatory of Hamilton College, Clinton, N.Y. By C. H. F. Peters. Charts Nos. 1–20. Astronomers will greet this first instalment of Professor Peters' long expected star maps with much pleasure. Already in August 1873 Professor Peters exhibited some of his maps and explained their construction at the meeting of the Astronomische Gesellschaft in Hamburg. They are the results of many years' zone observations of stars down to the 12th or 13th magnitude in search of minor planets. The Zones were observed in about the same way as the Harvard Zones, and the charts constructed from them were each compared with the heavens on three evenings. The epoch of the charts is 1860 and as none of the charts comprise regions further than 30° from the equator, the same uniform projection could be employed throughout, the scale adopted being 60 millimetres to a degree (about the same as Chacornac's). A star's coordinates may be got by the chart with an uncertainty of only 0′·2 resp. 3ˢ. Each chart covers 20^m in R.A and 5° in Decl. and is intended to give as complete a picture of the sky as a power of 80 on the 13-inch refractor offers. In the margin of each map are given the scale of magnitudes, references to places where minor planets were discovered by Prof. Peters, the date of the last revision and the precession with opposite sign for the middle of the map. As our readers are already aware (see Vol. I, p. 51) the charts contain a large number of nebulæ, many of them being among the faintest of those found by d'Arrest, Marth and others. The charts are photolithographed and are extremely well looking.

Étude du Spectre Solaire par Ch. Fievez. (Bruxelles 1882, 4 pp. 4to and seven plates.) This publication contains a map of the Solar Spectrum from λ 6600 to λ 4500 and in this space it shows 2100 lines while Ångström's normal spectrum has only 980. This is owing to the very great dispersion which M. Fievez succeeded in obtaining by combining a Christie half-prism spectroscope with a Rutherfurd grating of 17276 lines to 25^{mm}. The grating was placed between the collimator and the prisms so that it could turn round a vertical axis situated in its plane and parallel to the lines, the optical axes of the collimator and of the observing telescope being in the same horizontal plane and placed so as to form as small an angle as possible (about 20°). The combination which gave the best results was that of the grating-spectrum of the second order with two half prisms, which gave an angular distance between the D-lines equal to about 44′. M. Fievez gives a list of 32 of Ångström's lines, common to two metals, which he has resolved into two lines, and in two cases into three. Nearly all of these lines had already been resolved by Young (Amer. Journ. Nov. 1880) and by Liveing and Dewar (Proc. R.S. No. 213). Besides these, M. Fievez has resolved 35 other lines. The charts are based on Ångström's, the new lines being interpolated between the old ones.

Zur Beurtheilung der Veränderlichkeit rother Sterne. Von E. Lindemann. (Mém. de l'Acad. Imp. de St. Pétersbourg. T. XXX. No. 4, 10 pp. 4to). In order to be able to compare observations made with a Zöllner photometer with those made by other observers it is necessary for each observer to determine his so-called curve of intensity. A star is selected as standard and is compared with the artificial star in all the colours which the colorimeter can give the latter. By comparing the results found by Zöllner, Rosén, Wolff and himself, M. Lindemann finds, that while the brightness which the various observers must give the artifical star in order to make it equal to that of a white Standard Star, is about the same for white, bluish, yellowish and yellow stars, they differ very considerably in the case of orange, reddish and red stars, even more than one class of magnitude. As the curves determined in three different years by M. Lindemann with the same instrument differ a good deal *inter se,* the differences cannot arise from the instruments, and experiments made in Pulkova and Potsdam have proved that the light which produces the artificial star is not the cause either, as gas, oil and various kinds of paraffin gave almost identical results. The phenomenon must therefore arise from physiological differences in the eyes of different observers and in the eye of one observer at different times. It has been found by experiments by Lamansky and Dobrowolsky (see Aubert's "Grundzüge der physiologischen Optik," Leipzig 1876 p. 531) that the eye is three or four times less sensitive to differences of brightness in the red and orange part of the spectrum than in the remaining parts. As estimations of the brightness of red stars, and especially of red variable stars, are generally made by comparing them with white or bluish stars, this phenomenon is deserving of special attention, and it seems doubtful, whether the prevailing opinion, that red stars furnish the largest contingent to the variable stars, may not to a great extent arise from physiological peculiarities of the eyes, at least where the supposed variability does not exceed one class of magnitude.

Ueber den Einfluss der Diffraction an Fernröhren auf Lichtscheiben. Von Hermann Struve. (Mém. de l'Acad. de St. Pétersbourg T. XXX., 104 pp. 4to.) The influence of diffraction on the images of luminous points in a telescope was first investigated by Airy and independently by Schwerd about fifty years ago, but the extension of the theory to the images of discs was not entered on, though Schwerd pointed out the principle to be adopted. Since then, the study of the subject has scarcely made any progress except that experiments by M. André have thrown much light on the nature of various diffraction phenomena, especially the "black drop" during Transits of Venus and Mercury. The theory of telescopic diffraction, and particularly of its influence on images of discs, forms the subject of the extensive memoir by M. H. Struve, the title of which is given above. The chief part of his investigation deals with the determination of the intensity of a disc of infinite extent in all directions, or of the intensity of a point situated at an infinite distance from the edge, inside the luminous disc, the latter being of limited extent on one side. Geometrically this problem comes to the same as the determination of the volume of an infinite body of revolution, and is solved in a very complete and simple manner. This leads next to the determination of the intensities both at a greater distance as also close to the edge of a disc bordered by a straight line or by a circle. The experiments hitherto made to deduce from observations the influence of diffraction on planetary discs or artificial discs, and lastly the theory and experiments on the black drop are discussed at some length, after which the two last chapters consider the distribution of light along a luminous line and the application of the theory of diffraction to the heliometer.

Although it had been supposed by several astronomers, that the diffraction in the object-glass could influence the phenomena of the black drop, the credit of having pointed out its great influence on observations of artifical Transits and of having investigated it theoretically, belongs to Professor Bakhuyzen, whose paper on this subject was published in the summer of 1874 (Astr. Nachr. Vol. 83 No. 1988). His observations were made with several telescopes, the apertures varying from

2 to 7 inches, and the formation or breaking of the black drop appeared (within the limits of the errors of observations) simultaneously for the various telescopes. That the spherical aberration was not the principal cause, was proved by putting diaphragms down to two inches aperture on the seven inch glass, as this made the ligament considerably broader instead of making it narrower. This broadening could not arise from the decrease of light of the image, as a tinted glass held between the eye and the eyepiece (with full aperture) did not produce it. An alteration in the focussing of the eyepiece did not change the breadth of the ligament and only influenced the sharpness of the image. When a screen with an opening of a few tenths of a millimeter was placed before the eye (whereby the breadth of the pencil falling on the pupil and the influence of unequal refraction in various parts of the eye would be diminished) the ligament also became broader, which Bakhuyzen attributed to the increase of diffraction in consequence of the diminution of the pencil of light.

A full description of the black drop as seen on a model has lately been given by MM. André and Angot (Origine du ligament noir dans les passages de Venus et de Mercure, Annales de l'École normale, 1881), and the results of Prof. Bakhuyzen have hereby been confirmed and extended. Their experiments (which chiefly were made with electric light) showed, that not only the breadth, but also, to some extent, the length of the ligament was increased by a reduction of the aperture, which agrees with a result found on another occasion, that luminous discs of very great intensity increase their diameter with a decrease of aperture. They also found that the first disturbance at second internal contact occurred earlier for smaller aperture ; for a 6-inch telescope it appeared from 50^s to 55^s before the geometrical contact, for a 4-inch about 65^s and for a 2-inch 130^s before the contact. That observations of the real transit do not show such a clear dependence on the aperture, is however, well known. But, as remarked by M. Struve, it is of great interest to see, how strongly marked a special phase of the phenomenon was ; namely at the first internal contact "le moment où, la teinte de la partie centrale n'ayant point encore diminué, les bords de ce ligament devenaient rectilignes au voisinage du Soleil," and at the second internal contact "le moment où, la partie centrale du ligament ayant pris son maximum de teinte, ses arêtes contiguës au Soleil commencent à se courber." After some practice MM. André and Angot found, that this moment could be observed with the model within about a second, and that it corresponded almost exactly with the moment of true contact, as registered electrically. As shown theoretically by M. Struve, the curves of constant intensity which form the outlines of the ligament, must be convex towards the line joining the centres and have their minimum curvature at the geometrical contact. The above experience confirms this and furnishes a further proof of the general correctness of the explanation of the phenomenon as caused by diffraction. It is also in accordance with theory that a dark disc of a six times smaller diameter, as Mercury, should show a comparatively broader dark band, for, as also shown by M. Struve, the length of the ligament must be the same for various planets, while the breadth is proportional to the square root of their diameters. It is of importance, that MM. André and Angot have confirmed their direct observations by the means of photography, as this proves the objective character of the phenomenon and renders its explanation as caused by irradiation, peculiarities in the eye, &c., untenable. The photographs show Venus at the time of geometrical contact at some distance inside the Solar limb, connected with it by a bark band which is the longer and narrower the longer the exposure (provided the intensity be the same).

However, all this only refers to the model, and as observers of the last two transits are aware, the phenomena at real transits are far more complicated. Nobody seems to have seen a real " black drop " as black as the planet itself; most observers saw a shade-like object or some similar disturbance of the limb; some saw nothing whatever remarkable. As pointed out by MM. André and Angot, the different degree of intensity of the light after passing through the dark glass may cause the ligament to appear differently to different observers (as is the case with the model), but it seems very doubtful indeed whether the discrepancies can really, even in part, be accounted for in this way. After all, the chief point, on which observers differ, is not the appearance of the ligament but the length of time it lasts. When all the observations of the Transit of 1882 shall have been published it will be interesting to see whether observers agree better on this point than on former occasions.

Kurzer Bericht über meine Untersuchungen über die Hypothese eines wiederstehenden Mittels. Von O. Backlund (Mélanges mathém. et astr. T. VI., 4 pp. 8vo). The author states the results of his researches on Encke's comet in the following words :

> "The investigations hitherto made of the theory of Encke's comet really prove nothing as to the existence of a resisting medium in space. Even if we should succeed by such a hypothesis to explain sufficiently the increase of the mean motion and the decrease of the excentricity during the period 1819–48, a simple hypothesis like this will not at the same time suffice for the motion of the comet after 1865, as the variation of the mean motion after that time has most probably become different. Not until the period 1865–81 and its connection with the earlier one have been fully discussed, will it perhaps become possible to find indications of the nature of the unknown forces which act on the comet."

Bestämmandet af en Planets massa medelst iakttagelser af satelliternas indbördes lägen. Af O. Backlund (Proc. R. Swedish Acad. 1882, 10 pp. 8vo). Dr. Backlund is engaged on a new determination of the mass of Jupiter from heliometer observations of the distances and position angles of the satellites *inter se,* and not, as usual, from the planet. It is evident that measures of the star-like satellites from each other are far less likely to be affected by constant errors than measures from the planet are. In the present paper Dr. Backlund sets forth the formulæ by which the mass of the planet can be deduced from observations of this kind. Starting from Marth's formulæ (Astr. Nachr. Vol. 44, p. 115) for deducing from the elements of the orbit the distance s and position angle p of the satellite from the centre of the planet, he determines the mutual distances and positions as follows :—

In the spherical triangle between the pole Π, the planet P and the satellite S_1 we have $PS_1 = s_1$ and $\Pi PS_1 = p_1$, the angle $\Pi S_1 P$ we call $180° - p_1'$. The corresponding quantities for S_2 we mark with the index $_2$, and for S_2 with respect to S_1 they are called σ, π and $180° - \pi'$. The angles and their opposite sides in the triangle S_1PS_2 are thus—

$$\underset{S_1}{180° - (p_2' - \pi)}\,; \qquad \underset{S_2}{p_1' - \pi'}\,; \qquad \underset{\sigma}{p_2 - p_1}$$

By using Gauss' equations and supposing the apparent distances to be so small as to enable us to put $p_1 = p_1'$, $p_2 = p_2'$ and $\pi = \pi'$, the following formulæ will be found—

$$\begin{aligned}\sin \tfrac{1}{2}\sigma \sin \pi &= \sin p_1 \sin \tfrac{1}{2}s_1 \cos \tfrac{1}{2}s_2 - \sin p_2 \sin \tfrac{1}{2}s_2 \cos \tfrac{1}{2}s_1\\ \sin \tfrac{1}{2}\sigma \cos \pi &= \cos p_1 \sin \tfrac{1}{2}s_1 \cos \tfrac{1}{2}s_2 - \cos p_2 \sin \tfrac{1}{2}s_2 \cos \tfrac{1}{2}s_1\end{aligned}$$

In deducing the differential formulæ through which the corrections to the elements and mass are to be determined, we can instead of $\sin \frac{1}{2}\sigma$, $\sin \frac{1}{2}s_1$, $\sin \frac{1}{2}s_2$ put the respective arcs and put $\cos = 1$. We thus find—

$$\left.\begin{aligned}\sigma \sin \pi &= s_1 \sin p_1 - s_2 \sin p_2\\ \sigma \cos \pi &= s_1 \cos p_1 - s_2 \cos p_2\end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (1)$$

These formulæ may also be found by considering the triangle S_1PS_2 as a plane triangle and project the broken line PS_2S_1P on the direction $P\Pi$ and its perpendicular.

If we now put—

$$\begin{aligned}s_1 \sin p_1 &= x_1 \qquad & s_2 \sin p_2 &= x_2\\ s_1 \cos p_1 &= y_1 \qquad & s_2 \cos p_2 &= y_2\end{aligned}$$

we find from (1)—

$$\left.\begin{aligned}d\sigma &= \sin \pi\,(dx_1 - dx_2) + \cos \pi\,(dy_1 - dy_2)\\ \sigma d\pi &= \cos \pi\,(dx_1 - dx_2) - \sin \pi\,(dy_1 - dy_2)\end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \quad (2)$$

Dr. Backlund next introduces into these formulæ Bessel's coefficients A, B, C, &c.,* and finally finds expressions for $d\sigma$ and $\sigma d\pi$ in which the corrections to the elements appear as coefficients. As the number of unknown quantities is twelve, six observations at least are required in order to obtain all the corrections to the elements. The correction to the adopted mass being the principal quantity sought, the Author proceeds to show that measures of the satellites *inter se*, quite apart from the observations being more accurate, are of greater advantage for the determination of the mass than measures from the planet are.

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* Bestimmung der Bahn des Hugenischen Saturn Satelliten.

même moins déviée qu'elle ne l'est au périhélie (exemples: comètes 1811, 1744, 1861 II, comète de Halley en 1835, et encore quelques autres qu'on trouvera dans mes recherches).

Dans les comètes à courte distance q, dont les anomalies lors la visibilité sont très grandes, la différence entre la position possible de la queue du premier type et la direction de la développante dévient très grande, et la queue-enveloppe prédite par M. Schwedoff avant le périhélie de la comète Wells est tout à fait impossible.

Les personnes, qui evitent le calcul et les mesures et qui se contentent des réflexions vagues, pourraient confondre dans quelque comète la queue-enveloppe après le périhélie avec la queue du III type, qui est toujours courte et très déviée du rayon vecteur.

Quant à la queue-enveloppe d'une comète à petite q avant le périhélie où elle doit avoir le plus grand éclat, elle précède le prolongement du rayon vecteur et fait avec lui un angle très grand, en restant toujours dans l'angle formé par l'axe de ξ positif et l'axe de η négatif. Or, une queue pareille n'a jamais été vue depuis que les astronomes observent les comètes, et elle n'a pas été vue dans la comète de Wells.

On tachera peut-être, en invoquant la perspective, d'identifier la queue-enveloppe avec ce gonflement du bord antérieur près de la tête que nous avons observé longtemps dans la comète Wells et qu'on voit sur les croquis. Calculons donc la position apparente de la queue enveloppe par rapport à l'axe de la queue principale. Calculons aussi la position de la queue anomale, qui se trouve toujours, quand elle existe, dans l'angle formé par les axes négatifs des ξ et des η (on a observé une telle queue, dirigée vers le Soleil, dans le comète 1882 II).

Supposons que l'angle ϕ' de la queue anomale est + 225° ou — 135°. Faisons notre calcul pour le 21·5 avril, 11·5 mai et 27·5 mai. Soient les angles de position : pour la queue principale p°, pour la queue-enveloppe p, et pour la queue anomale p'. En n'oubliant pas que la queue-enveloppe est perpendiculaire à la tangente, nous aurons—

	v	ϕ	P	S	p°
Avril 21·5	— 156°·3	281°·9	351°·6	95°·8	255°·6
Mai 11·5	— 151·6	284·2	49·5	117·8	304·0
„ 27·5	— 141·5	288·8	88·8	135·7	351·4

et puis—

	p	p'	$p^\circ - p$	$p^\circ - p'$
Avril 21·5	90°·1	81°·5	165°·5	174°·1
Mai 11·5	197·2	146·3	106·8	157·7
„ 27·5	280·6	205·2	70·8	146·2

D'après ces angles $p^\circ - p$ et $p^\circ - p'$ on voit que la comète 1882 I n'avait ni la queue anomale ni la queue-enveloppe.

Et où sont ses développantes dans les brillantes comètes de 1680 et de 1769, toutes deux à courte distance périhélie? Et laquelle des deux queues de la comète 1865 I coïncidait avec la développante ; et la même question pour la comète 1843?

Il est très intéressant de savoir comment dans la théorie des ondes cosmiques sera expliquée la forme conoïdale de la racine de la queue et le rebroussement des jets dans le cas où la comète s'éloigne du Soleil presque en ligne droite, comme, par exemple, dans la comète 1882, II. Et la figure des queues dans les plans perpendiculaires à l'orbite, et les queues anomales, et maintes autres choses. Du reste l'onde est mobile, et on peut la faire jaillir partout où l'on veut.

M. Schwedoff a donné aussi la forme typique de la comète, dont la distance périhélie n'est par trop courte (Théorie mathématique des formes cométaires ; planche VI., fig. 21.), ou dont le mouvement fait en général un angle assez grand avec le rayon vecteur. Les astronomes n'ont jamais vu une pareille queue-monstre ni dans la nature, ni dans les esquisses qui ont quelque valeur. Probablement c'est l'exagération de quelques dessins mal interprétés et mal confrontés (les traces laissées sur la planche lithographique par les plis de son papier paraissent quelquefois appartenir à la figure imprimée; un observateur voit les particularités du phénomène, tandis qu'un autre en même temps l'observe dans tout son ensemble etc.).

Maintenant j'ai entre mes mains un manuscrit, dont l'auteur tache de démontrer que les queues des comètes sont des *trombes*, produites par le mouvement ou plutôt moyennant le mouvement parabolique d'un noyau de comète dans le milieu interplanétaire.

Voilà encore une spéculation où le point de départ est une simple ressemblance superficielle. Cette ressemblance—on l'érige en théorie, on la munit de quelques formules mathématiques et on la tire par les cheveux pour la mettre d'accord avec les faits, dont plusieurs restent inconnus à l'auteur. Et ce qui est ridicule, c'est qu'on tache avant tout d'entamer une polémique.

J'ai preparé le champ et j'ai aplani le chemin à tous ces faiseurs des théories. Pour une trentaine de comètes j'ai calculé les coordonnées de leurs queues dans le plan de l'orbite et j'ai taché de recueillir toutes les notes concernant la structure de la queue et de la tête, la stratification, la distribution d'éclat, en somme toutes les particularités qui peuvent servir comme pierres de touche à différentes spéculations. Les théoriciens auraient dû premièrement faire passer leurs raisonnements par ces épreuves. Si la théorie en sort avec succès, elle sera la bienvenue.

Moscou, Décembre, 1882.

PRIZE QUESTION OF THE ROYAL DANISH SOCIETY OF SCIENCE.

The number of Minor Planets between the orbits of Mars and Jupiter has by degrees become so large, that it is not to be expected that it will in future be possible to compute in advance the motion of every single one. And it will even be less possible to compute their influence singly on the motions of the major planets or of comets. Fortunately, however, the masses of the minor planets are so trifling, that the perturbation caused by any one separately may be left out of consideration; but it is very doubtful whether their collective influence might not be traced in the motion of the nearer planets or comets. But in order that researches on this point should give a reliable result, it is necessary first to know the form and position of the ring formed by all the minor planets, and the distribution of the masses in this ring.

No degree of accuracy can be attempted in the statistical description of the ring, and with very few exceptions the systems of elements already deduced for each planet may be adopted; the more so as it will be of no importance, whereabout in its orbit a planet is at any time. As to the single masses, it is of course necessary to draw conclusions from the apparent brightness, but the number is so considerable that a fairly reliable result may be hoped for. In the statistical researches hitherto made the separate elements only have been discussed, apart from their connexion with the other elements, but this cannot be considered satisfactory. Thus the fact that the planets, arranged according to their mean distances, are divided into a number of distinct groups, does not by any means prove, that the ring formed by them around the Sun is dissolved into a number of fairly concentric rings.

The Society therefore offers its Gold Medal (value 320 crowns equal to nearly £18) for a statistical investigation of the orbits of the Minor Planets, considered as parts of a ring around the Sun. The form, position, and relative distribution of mass, should if possible be stated with at least so much accuracy as is judged necessary for computing its perturbing influence on planets and comets.

[Answers to be written either in the Latin, French, English, German, Swedish, or Danish languages and to be sent before the end of October 1884 to the Secretary of the Society, Dr. H. G. Zeuthen, Copenhagen. They should not bear the Author's name but only a Motto, while the name should be enclosed in a sealed envelope.]

RECENT LITERATURE.

Bestimmung der Masse des Planeten Jupiter aus Heliometer Beobachtungen der Abstände seiner Satelliten. Von Dr. W. Schur (Nova Acta der Leop. Carol. Akad. Band XLV., Halle, 1882, 77 pp. 4to). The results of the investigation described in this valuable paper have already been given in this journal, Vol. 1 p. 201.

Logarithmisch-trigonometrisches Handbuch auf fünf Decimalen bearbeitet von E. Becker. Leipzig 1882, XVI. and 104 pp. 8vo (Tauchnitz, Stereotyped edition). These tables are beautifully printed with old face types on fine paper and contain: Briggs's logarithms of numbers from 1 to 10000 in 21 pp. of double entry, the logarithms up to 1000 being given in full, and the remainder with the first figure detached; the average differences as well as tables of proportional parts are shown on the margin, and the logarithms S and T for transformation into *minutes of arc* are put at the foot of each page. P.22 contains the natural trigonometrical functions for each degree to four places. The most novel table of the book is that on pp. 24–35, which gives the log sin and log tang for every *tenth of a minute* for the first 6° of the quadrant; here too the mean differences are given, but without auxiliary tables. A table of the lengths of arcs for degrees and minutes occupies p. 36. The logarithmic trigonometrical tables for the whole quadrant at the rate of 1° to a page follow next, the differences and proportional parts being supplied from the 6th degree. Addition and subtraction logarithms on Zech's plan fill pp. 83–97, and then two pages of squares of 1–1000, a page each for the transformation of arc into time and into arcual seconds, and finally three pages of useful constants complete the book. The most useful feature of the book is the keeping of the minute as the lowest unit in the trigonometrical part. The most scrupulous care has been taken to render the tables correct.

Observations of the Transit of Venus made at the Washburn Observatory, Madison, Wisconsin, 1882 *December* 5–6. *By E. S. Holden* (Amer. Journal 1883, 3 pp.). The Transit was observed at Madison by Prof. Holden, using the 15½-inch Clark Equatoreal with the aperture cut down to 6 inches, and by Mr. G. C. Comstock using the 6-inch Equatoreal formerly belonging to Mr. Burnham. On the former telescope a positive eyepiece having a power of 195 was fitted to a Clark reflecting solar eyepiece, the latter was furnished with a similar eyepiece magnifying 176 times. The large telescope was situated in Long. W $= 0^{h}\ 49^{m}\ 25^{s}\cdot78$, Lat. $= +\ 43°\ 4'\ 36''\cdot8$, the smaller one was $0^{s}\cdot18$ east and $0''\cdot7$ north of it. The following observed times are expressed in Chicago M. T. ($7^{m}\ 11^{s}\cdot11$ later than local M. T.)—

		Holden.		Comstock.
External contact		$20^{h}\ 14^{m}\ 28^{s}\cdot2$		$20^{h}\ 14^{m}\ 46^{s}\cdot6$
Internal –	a	33 24·0	a'	33 15·4
	b	33 53·9	b'	33 26·4
	c	34 11·8	c'	33 37·3
	d	34 24·8	d'	33 42·3

The moment (a) was supposed to be the time of second contact; at (b) and (c) the ligature persisted, but in both of these phases the contour of Venus prolonged by the eye was inside the limb of the Sun, especially so in (c). The cusps met at (d) but did not persist until about 11^{s} later. Mr. Comstock saw at (a') a bright line of light shoot across from cusp to cusp, at (b') another was seen, and at (c') the cusps seemed to meet for the first time; at (d') the contact was considered past. The third and fourth contact were lost, a snow storm having set in about 22^{h}.

Madeira Spectroscopic, being a revision of 21 *places in the Red Half of the Solar visible Spectrum with a Rutherfurd Diffraction Grating at Madeira during the summer of* 1881. *By C. Piazzi Smyth.* (Edinburgh 1882, 42 pp. and 18 plates, 4to). Having procured two fine examples of Rutherfurd Diffraction Gratings with 17296 lines to the inch, over a surface 1·6 inch square on speculum metal, Professor Piazzi Smyth proceeded to Madeira in May 1881 in order to benefit by the favourable skies at that island in studying certain parts of the Solar Spectrum. At a station situated about 270 feet above the Sea Level the instruments were set up, consisting of a heliostat carrying a silvered mirror, from which the Sun's light was reflected through a 6-inch achromatic lens of 90 inches focus and then through a large separating prism of white flint of small refracting angle, to the slit. Through a collimating lens of quartz, 2·25 inches in diameter and 31 inches focal length, the rays passed on to the grating and were viewed in reflection from that by a telescope with a 2·25 inch quartz objective of 52 inches focus and eyepieces magnifying 30, 45 and 64 times. Either the second or the third order of spectrum furnished by the grating was employed. "The observations almost necessarily took the shape of critical revisions of crucial groups of lines as already measured, pictured and mapped by various standard authorities, besides comparing them directly with the living spectrum." To this end sets of measures were made of each group as seen in the Sun, at both high and low altitudes on several occasions, and from these measures two drawings of each group from different days of observations were prepared and again compared with the Spectrum. The authorities compared were: Brewster and Gladstone, Ångström, Kirchhoff and Hofman, Rutherfurd (probably Plate VI. in Secchi and Schellen's "Die Sonne"), an engraving by Janssen (from *C* to *D*), Hennesey (Phil. Tr. 1875), Abney's photograph of the ultra-red end, Langley's views of *A* and *B*, Fievez's drawing of *b*, Vogel's map and a pencil sketch by Young of *E*. The plates exhibit the results of five of these authorities and two Madeira determinations of twenty-one special parts of the spectrum, and the main part of the text consists of explanatory notes to the Plates. The drawings are very peculiar looking, the intensity of the lines and the various shadings being represented symbolically in a very simple manner by lines of different height and slanting lines, which precludes the chance of thin lines representing a shade or band being mistaken for isolated thin Frauenhofer lines. The unit employed is, as in other publications by the same Author, the British inch, and a plate facing the title page contains three very instructive representations of the spectrum, according to wave-numbers (the red and the violet being of about equal extent), according to wave-lengths (contracting the violet) and as shown by an average of prisms (contracting the red.) This very remarkable work is beautifully printed and must, like Professor Smyth's former publications, rank among the most important works on the Solar Spectrum ever published.

The illustrated Account given by Hevelius in his "Machina Celestis" of the

Method of mounting his Telescopes and erecting an Observatory. Reprinted from an original copy with some Remarks. By C. Leeson Prince. (1882, Printed for private circulation, 80 pp. and 10 plates.) As indicated by the title, this neat little volume contains a reprint, followed by a translation, of three chapters (18, 21 and 22) of Vol. I of the "Machina Cœlestis" of Hevelius, which describe his arrangement for viewing Sun-Spots by projection, his 150 feet telescope and his proposal for a revolving tower for supporting very large telescopes. The Editor has to these chapters added a short account of Hevelius' and Gassendi's observations of planets and the observation of Saturn by the brothers Ball, which has until quite recently been misunderstood as proving them to be the first discoverers of the division of the ring. Portraits of Hevelius and Gassendi, five plates with reduced copies of some of the plates in the "Machina Cœlestis," and two pictures with views of the instruments at Mr. Prince's own observatory, accompany the book. Although the *first* volume of the "Machina" is not quite as scarce as supposed by Mr. Prince, it is at any rate not easily accessible to many astronomers, and to such the pleasant little book is therefore very acceptable.

Neue Reduction der Bradley'schen Beobachtungen aus den Jahren 1750 bis 1762 von Arthur Auwers. Zweiter Band. (St. Petersburg 1882, 412 pp. large 4to). The second volume of this long-expected great work has been published before the first one, because it was simpler to finish the MS. of it before that of Vol. I. It contains the single results obtained with the Bird Transit Instrument and the so-called new Bird quadrant in the period from 1750 Aug. 21 to 1762 July 16, also the resulting mean RA. and Meridian Zenith Distance for 1755, except the RA. of Polaris and of the 36 Standard Stars of Maskelyne. The new Catalogue of 3268 stars for 1755·0 and its comparison with modern observations is to form the contents of the third volume, which is to appear next, while the necessary details as to the new treatment of the materials, the reduction of the observations of the Sun, Polaris and the Standard Stars, and lastly the reduction of the observations made with the Zenith Sector are reserved for the first volume. In the present volume the Right Ascensions and Zenith Distances are separated in two catalogues, containing the date, the name of the observer, the reduction from apparent place to 1755·0, the mean RA. or ZD. for this epoch and the weight, for the single observations of every star, under which the mean epoch of observation and the mean resulting RA. or ZD. are given. The Zenith Distances in the upper and lower culminations are given separately. The correction to Bessel's Equinox ($+ 0^{s}{\cdot}056$) is not included in the RA.'s as printed. The stars are designated by the usual numbers. Professor Auwers has detected a considerable number of errors in the observations (*i.e.*, in the carefully prepared copies which have been at his disposal) in addition to the errors found by Bessel and Rigaud. A list of these precedes the catalogues, the more important or difficult cases being discussed at some length.

Thirty-seventh Annual Report of the Director of the Astronomical Observatory of

Harvard College. By E. C. Pickering (15 pp. 8vo). Among the observations made with the 15-inch Refractor are systematic sweeps for objects having singular spectra. These sweeps have confirmed the well known fact (first pointed out by d'Arrest in 1872) that gaseous nebulæ very rarely are met with outside the Milky Way and never at a considerable distance from it. A special search was therefore made last summer in the Milky Way from − 30° to + 20° Decl., which resulted in the discovery of seven new minute planetary nebulæ; one of them being D.M. + 1°, 3979. The Satellites of Mars were at the last opposition only observed photometrically; the difference of brightness of Deimos when preceding and when following Mars, noticed in 1877 and 1879, was not now apparent. The brightness of various points on the Moon has been determined (Selenogr. Journ. V., 57). It appears from this investigation, that the scale of brightness in common use among observers of the Moon might be very clearly expressed in terms of stellar magnitudes. each degree of the scale answering to the ratio of light equivalent to six-tenths of a magnitude. A modification has been made of Professor Pritchard's wedge photometer, suitable to the measure of the light of faint stars in zones. The wedge is placed so that the diurnal motion carries the stars from its thin to its thick portion, and the time of disappearance is noted. A bar in the unobstructed part of the field serves to determine the position of the star. The stars in Bond's zone + 0° 50′ to + 1° 0′ Decl. are being re-observed in this way, partly to determine their light, partly to detect cases of Proper Motion.

The measurement of the light of the stars visible to the naked eye with the Meridian Photometer was completed last summer. Over ninety thousand measures were made on about four thousand stars, most of them being observed on from three to six nights, four settings being made every night on each object. A series of estimates by the unaided eye have also been made by three observers for purposes of comparison. Among problems of general interest connected with the work is the atmospheric absorption. A discussion of about fifteen thousand observations available for this investigation shows that we may assume that the absorption, at any altitude exceeding 15°, equals in stellar magnitude one-quarter of the secant of the Zenith Distance. This agrees very nearly with the empirical law deduced by Seidel, especially if we apply a correction for the low barometric pressure due to his great elevation. The constancy of the Pole Star is established by these photometer observations. Eye estimates were also made to compare the light of the Pole Star with that of other stars of nearly equal brightness near it, and show that the relative position of two stars to be compared has an important influence in their relative brightness. A reduction of the observations of Sir W. Herschel on stellar magnitudes has been effected, and has led to interesting results. It appears that the intervals Herschel designated as a period, comma and dash, do not exceed one, two and four tenths of a magnitude, and that the average deviation of a single comparison of two stars is only 0·25 mag. As this includes the error of the Harvard measurement of each star, the difference of each as seen by the eye from its true brightness, and the variation each has undergone during the past century, the errors of Herschel's observations must be very small, and his measures must be of the utmost value in determining changes of long period.

Mémoire sur la Grande Comète australe du mois de Février 1880, *par M. Wilhelm Meyer.* (Mém. de la Soc. de Physique de Genève T. XXVIII., 36 pp. 4to). At the time when only the provisional results of some of the observations of the Great Southern Comet of 1880 were available for determining the orbit of this body, M. Meyer found by repeated trials that it was impossible to satisfy the observations by any ellipse of a semi axis major equal to 11·086, with which value, suggested by the resemblence of the orbit to that of the comet of 1843, he had commenced his researches. He took up the work anew, when the observations made at Cordoba, the Cape and Melbourne had been definitively reduced, and forming from them seven equidistant normal places for every second day from February 6·0 to February 18·0 and deducing the differential formulæ by the method given by Oppolzer (Lehrbuch II. pp. 405–407) M. Meyer finds the most probable elements—

$$\mathrm{T} = 1880 \text{ Jan. } 27{\cdot}481398 \text{ M. T. Berlin.}$$
$$\left.\begin{array}{rcrrrl} \text{☊} &=& 356^\circ & 18' & 33''{\cdot}70 & +\ 0''{\cdot}5327\,de \\ \omega &=& 77 & 55 & 26{\cdot}30 & +\ 0{\cdot}4626\,de \\ i &=& 143 & 7 & 49{\cdot}69 & +\ 0{\cdot}1092\,de \end{array}\right\} \text{M. Eq. } 1880{\cdot}0.$$
$$\log q = 7{\cdot}7713850$$
$$e = 0{\cdot}99946804$$
$$\mathrm{P} = 37{\cdot}004 \text{ years.}$$

de is here expressed in unities of the eighth decimal of *e*. The period of revolution found in this way certainly seems to be a strong argument in favour of the identity of the comets of 1843 and 1880, but it is evident that the very small heliocentric arc through which the latter comet passed during the fifteen days it was observed, must make the period of revolution very uncertain, and it does not appear that Dr. Meyer has tried to represent the observations by an ellipse of 533 years, the period found by Hubbard for the comet of 1843. He has found the mean error of an observation of 1880 to be 3″, and that the period of revolution will lie between 31·5 and 47·7 years if the mean error be increased to 4″. Considering, however, how little accuracy the observers themselves attributed to their results, and that the number of observers was only four, so that personal errors may not be quite eliminated, the above limit might possibly be much extended. In the case of the comet of 1843 the mean error of a single observation was 8″·4, and on the whole it can hardly be said that Hubbard's orbit precludes the possibility of a different value of the period satisfying the observations, so that a renewed discussion of the observations from 1843 seems desirable. The curious fact, that the great comet of 1882 appears to move in a very similar orbit, would rather appear to point to the existence of a number of cometary bodies moving in the same track, being that originally followed by a larger comet from which they have, by degrees, become detached.

CHRISTIAN MAYER'S OBSERVATIONS OF TELESCOPIC STARS, ARRANGED ACCORDING TO CONSTELLATIONS.

Some years ago I prepared for my own use a list of all the distant companions to bright stars which were observed at Mannheim by Christian Mayer with an eight feet Mural Quadrant in the years 1777 and 1778. The observations were published in a book entitled "De novis in coelo sidereo phaenomenis in miris stellarum fixarum comitibus Mannhemii detectis" (Mannheim 1779, 4to). They are given in chronological order, and partly on this account, partly because the book in question is rather scarce, I have thought they might be worth publishing again, arranged according to constellations. The single observations are given separately, and I have also given in the last column the page of the book on which each star is to be found, pp. 38–56 referring to the year 1777, and pp. 57–76 to 1778. The magnitudes and differences of Right Ascension and Declination are given below exactly as published by Mayer.

H. C. F. C. Schjellerup.

Copenhagen Observatory, Jan., 1883.

α Andromedæ.

	− 1^{m} 49^{s}	− 4′ 2″·8	49

β Andromedæ.

10	− 2 50	− 3 29·6	71
10	+ 0 11	+ 2 36	53
10	+ 0 17·5	+ 4 37	53

γ Andromedæ.

7	+ 0 0·75	+ 0 6	76
tel.	+ 0 2	+ 0 4·5	39

δ Andromedæ.

12	− 2 9	− 11 44·8	72
9	+ 1 31·2	+ 14 58·3	72
tel.	+ 4 46	+ 2 20·6	72
9	+ 7 1	+ 5 9	72
9	+ 7 4	+ 5 33	72

λ Andromedæ.

7	− 7 52·5	− 0 44·3	71
tel.	− 7 8	− 0 47·3	71
8	− 3 36	+ 12 8·6	71
7	− 3 35	+ 12 6	52
7	+ 0 20		52
8	+ 3 44	+ 22 27·0	71

ο Andromedæ.

8	+ 4^{m} 36^{s}·2	+ 19′ 30″·8	70

ζ Andromedæ.

9	− 6 57·2	− 13 29·3	71
10	− 1 2	+ 1 43	72
10	− 1 1·5	+ 1 44·1	71
10	+ 3 12	− 3 6·6	72

θ Antinoi.

12	− 4 10	− 1 48·7	72
7·5	− 3 16·5	+ 9 40·6	72
	− 3 16	+ 9 45·2	48
9	+ 0 47		48
7	+ 1 54·5	− 11 45·9	48
7	+ 1 55	− 11 45·7	72
7	+ 5 5	− 42 2	72

α Aquarii.

7	− 1 0	− 34 52	54
9	− 0 28	+	54

β Aquarii.

	− 6 48	+ 0 53	55

γ Aquarii.

8	− 5^{m}	3^{s}·7	− 11′	50″	68
	− 5	2·5	− 11	42	48
7	− 4	53	− 43	4	50
9	− 3	8			48
9	− 3	7·8	+		50
	+ 2	57·0	+ 11	29·8	48

η Aquarii.

	+ 0	59·0	−		53

θ Aquarii.

8	− 4	14	+ 13	7·7	68

ζ Aquarii.

	+ 1	23·5	+ 9	28·1	53

h_1 Aquarii.

7·5	+ 0	10	− 3	32·2	55
7	+ 0	33			55
	+ 0	45	− 14	34·3	55

λ Aquarii.

tel.	− 2	23	+ 7	36	55
	+ 1	58	+ 22	32·6	55
7·5	+ 2	37			55

φ Aquarii.

7·5	− 3	39·1	+ 4	48	51
8	− 3	38·5	+ 4	46·7	49
tel.	+ 0	54·3	− 5	12	51
tel.	+ 0	55	− 5	12·0	49

σ Aquarii.

tel.	+ 0	13			55
7	+ 1	1·5	− 13	45·7	55

ψ_1 Aquarii.

10	− 0	48·5			71
10	− 0	47	+ 9	56·4	71

ψ_2 Aquarii.

9	− 2	55	+ 15	48·8	71
tel.	− 2	54·5	+ 15	22·7	71

α Aquilæ.

8·5	− 7	53	− 25	31	47
10	− 7	37	+ 4	21	47
7·5	− 5	57	− 5	18	47
8	− 2	25	+ 2	2·5	47
9·5	+ 1	51	+ 16	39·6	47
7·5	+ 3	32	− 23	44	47
12	+ 3	39·5	− 5	19	47
12	+ 3	40	+ 19	30·6	47
7	+ 5	51	− 20	5·4	47
10	+ 6	11	− 25	30·5	47
	+ 10	16	− 20	8·2	53
12	+ 12	10	− 30	10·6	47

γ Aquilæ.

10	− 1^{m}	33^{s}	+ 53′	23″·6	66
7	− 1	32	+ 18	19	50
9	− 1	18·5	+ 10	6·6	66
8	− 1	17·5	+ 10	8	50
8	− 0	45			66
	+ 0	18	−		50
8	+ 2	15	+ 4	19·3	66
	+ 2	15	+ 4	9	50
6	+ 4	40·5	− 12	35·9	66

δ Aquilæ.

12	− 4	48	− 8	51·9	66
	+ 0	8	+		49
8	+ 2	53			66
7	+ 4	42	− 13	44·7	66

ε Aquilæ.

	− 0	25	+ 3	29·1	47

χ Aquilæ.

10	− 1	40	+ 6	24·7	68
8	− 0	9·2	+		68

α Arietis.

10	− 5	29	− 5	25	74
tel.	− 1	7·7	+ 0	13·7	74
	− 0	30·3	− 49	23·3	56
12	+ 3	43	+ 20	15	74
9	+ 5	24·3	+ 40	32	74
10	+ 8	1·5	+ 49	59	74

γ Arietis.

	0	0	+ 0	11·5	39
	+ 0	16·5	+		55

δ Arietis.

	− 1	59	− 21	10·3	56

ε Arietis.

8	− 0	22	+ 16	34·4	55

η Arietis.

9	− 1	25·7	− 9	48·1	74

θ Arietis.

7·5	+ 1	2	− 12	7·2	74

λ Arietis.

8	− 0	2	+ 0	25	72
8	+ 0	2·2	+ 0	24	76
10	+ 3	35·8	− 11	33·6	76

ρ Arietis.

6	− 0	35	+ 17	38·2	75
8	+ 1	32·5	− 12	59·2	75
8	+ 4	7	− 0	48·6	75

ξ Arietis.

9	— 1^m 35^s·5	+ 13′ 11″·6	75
8	+ 0 52	+ 5 10	75
8	+ 1 20·5	— 2 21·6	75
7	+ 3 31		75
tel.	+ 3 34		75

α Aurigæ.

7·5	— 0 16·5	+	54
tel.	— 0 10	+ 23 29·8	40
9·5	— 0 10	+ 7 10	54
10	— 0 9	+ 7 13	58
	+ 0 8	+	54
tel.	+ 1 7·5	+ 6 39	40
tel.	+ 1 8	+ 6 35·3	54
9	+ 1 18		58

α Bootis.

pallid.	— 2 17	— 12 10·5	38
10	— 1 59		62
10	— 0 52		45
8·5	— 0 51·2	+ 35 56·6	62
7	+ 0 5·7	— 23 33·6	62
7	+ 0 6	— 23 35	38
	+ 3 6·8	— 15 2·4	38
8	+ 3 8	— 15 0	62
8·5	+ 4 53		45
9	+ 5 9	+ 24 39·9	62
	+ 5 13·4	— 8 29·2	45
8	+ 5 13·5	— 8 30	62
9	+ 5 47	+ 31 3	62
12	+ 6 9	+ 28 58·2	62
12	+ 9 6	+ 10 34	62
tel.	+ 10 18	— 4 13·7	62
	+ 10 33	— 6 44	45
6	+ 10 33·5	— 6 43·1	62
8	+ 11 26	— 5 25·8	62

β Bootis.

9	— 9 38	— 1 54·9	44
	— 4 36·5	+ 13 11·5	44
tel.	0 0	+	44

γ Bootis.

	— 6 41·5	+ 7 3·4	38

δ Bootis.

8	— 6 25·1	— 10 59	39
10	+ 0 8	+ 0 20	39

η Bootis.

8·5	— 4 35	+ 13 18·7	43
8	+ 0 35	+	43

ζ Bootis.

10	— 0 27	— 11 33	39

β Cancri.

	— 0^m 17^s	+	60
12	+ 1 3	— 19′ 8″·3	60
	+ 1 9	— 1 1	60
	+ 1 34		54
	+ 1 42		54
	+ 1 52·1	+ 1 41·2	54
	+ 1 52·1	— 1 41	60
8	+ 3 57	+ 13 51·2	60

c Cancri.

8	— 5 12		43
	+ 1 0·5	— 4 32	42

ι Cancri.

	— 0 2	+ 0 17	44

κ Cancri.

	+ 1 11	—	53
	+ 2 7	—	53

φ Cancri.

tel.	+ 0 12		57

υ Cancri.

7·5	+ 1 29·5	+ 0 39·1	59
7	+ 1 29·8	+ 0 37·1	43

π Cancri.

7	— 2 49	+ 1 44·9	61
tel.	+ 0 38·5	+	44
tel.	+ 0 39	+ 4 12·5	61

ξ Cancri.

6	+ 0 59·5	— 2 48·1	61

θ Cancri.

10	— 3 42·1	— 2 40·2	74

ζ Cancri.

7	+ 0 0	— 0 7·2	74

ω Cancri.

10	— 2 8·5	+ 0 57·2	60
8·5	— 0 46		60

α Canum venat.

7	— 0 1	— 0 13	40

α Canis minoris.

10, neb.	— 4 24	+ 0 4·7	40
8	— 2 8	+ 6 21·1	52
10	— 2 8	+ 6 23·4	59
7	+ 0 39	— 3 6·5	40
9	+ 1 38		59
10	+ 3 8	— 30 24	59
8	+ 3 53·5	— 19 14·9	59
10	+ 9 54	— 9 26	59

α Canis majoris.

9	−	14^{m}	46^{s}	−	22′	34″·5	73
10	−	8	27	+	34	58	73
7	−	6	39·5	−	15	54	42
7	−	6	38	−	15	57·2	73
12	−	3	20	+	37	3·8	73
10	−	2	10				73
tel.	+	0	50	+	.....		73
10	+	0	50	+	.....		42
8·5	+	1	3				73
8	+	1	21	+	2	13	73
10	+	1	55	+	34	39	73
10	+	3	23	−	51	6·3	73
7	+	3	31	+	91	10	73
	+	3	31	+	91	11·2	42
7	+	5	2	−	37	13·1	73
7·5	+	5	5·5	−	24	54·5	73
7	+	5	6	−	24	51·7	54
10	+	5	14	−	9	26	73

δ Canis majoris.

8	−	1	22	−	16	36	52

ε Canis majoris.

	−	2	46	−	5	40·4	52
	+	0	28	+	.....		52
10	+	0	28	+	1	57·6	57

α^2 Capricorni.

7	−	6	4	+	11	21·1	51
8	−	5	40	+	26	22·6	51
7	−	1	39	+	15	43·8	51

α Ceti.

8	+	0	4	+	15	34·8	72
	+	0	4·5	+	15	30·3	39

β Ceti.

	+	4	12·5	−	4	13·4	50

δ Ceti.

	+	0	23·5	−	.....		56
	+	1	59·5	+	.....		56
7	+	2	24	+	13	43	76
8	+	2	28·7	+	13	29	76
	+	2	30·5	+	.....		56

ν Ceti.

10	−	2	24·5	+	5	52·4	70

ξ_1 Ceti.

7	−	1	36	−	16	32·3	73

ξ_2 Ceti.

8	+	1	55·8	−	2	21·2	76

θ Ceti.

	−	0	3	+	10	2·7	56

δ Corvi.

7·5	−	2^{m}	4^{s}	−	7′	28″·5	62
	−	2	2				56

α Cygni.

	−	1	58	+	23	47·9	53
12	−	1	6				67
7	+	5	51	+	16	34·2	67
8	+	5	52	+	16	31	53
10	+	6	55				53
12	+	6	55	+	8	36·5	67
12	+	6	57				67
6	+	11	43	−	8	50	67
tel.	+	11	55	−	.....		67

β Cygni.

10	−	10	30	+	0	14·1	46
8	−	6	18	+	9	35	46
	−	2	29				46
10	−	2	28	−	1	53	67
	−	2	13	−	1	20·6	46
8·5	−	1	36	+	0	12	67
6·5	+	0	2·1	+	0	19·9	67
7	+	0	2·5	+	0	17·6	46
12	+	1	23·5	+	17	59·5	67
10	+	3	29	+	26	17·5	67
9	+	6	29	+	30	43	67

γ Cygni.

8	−	10	18	+	7	10·6	64
	−	5	46	−	0	9·5	51
6	−	5	12	+	7	55·4	64
	−	0	47	+	.....		51
	−	0	4	+	.....		51
		0	0	+	.....		51
tel.	−	0	6·5				64
	+	0	34	+	.....		51
6	+	3	48·5	+	7	40·3	64
	+	3	49	+	7	38·3	51

δ Cygni.

10	−	1	5	−	.....		49
8	−	0	16	−	12	50·1	49
10	+	0	38	−	.....		49
10	+	0	40	−	19	0	49

ε Cygni.

12	−	2	42	−	4	14·8	68
12	+	0	58	−	4	29·9	68
tel.	+	0	58	+	.....		52

μ Cygni.

7	+	0	0·75	−	0	4·9	69
7·5	+	0	0·75	−	0	5·7	76
8	+	0	15·5				76
8	+	0	15·7	+	1	31·3	69
12	+	3	51	+	2	40	69
12	+	6	28	−	0	34	69
6	+	8	20·5	+	0	50·1	69

ζ Cygni.

7	− 4$^{\text{m}}$ 15$^{\text{s}}$·7	− 0′ 28″·8	69
6·5	− 4 15	− 0 31·6	51
	− 1 34·5	+	51
8·5	− 0 57·5	+ 23 28·2	51
8	− 0 57	+ 23 28·9	69
7	+ 1 11·5	− 20 2·2	69
	+ 1 12	− 20 0·6	51
12	+ 5 1	+ 4 33	69
10	+ 6 41	− 12 29·5	69
6·5	+ 9 39	+ 2 38·6	69

ω Cygni.

10	− 2 8	+ 11 4·2	70
9	− 9 19·2	+ 4 36·4	70
9	+ 0 1	+	70

β Delphini.

12	− 0 43	+ 7 45·9	69
10	− 0 41	+ 8 6·3	69

γ$_1$ Delphini.

9	− 0 28	− 13 47·3	70
3	+ 0 1·2	− 0 2·3	70
9·5	+ 0 57·3	+ 7 1·5	70
9	+ 2 55·5	− 9 14·7	70
12	+ 3 2·3	+	70
10	+ 4 26	− 6 27·3	70
9	+ 7 5	+ 10 43·2	70

δ Delphini.

8	− 0 33	− 4 12·2	68

ε Delphini.

Dupl.	− 2 1	− 2 8·2	49
	− 2 0	− 2 3·0	49

γ Eridani.

7	− 1 37·5	− 6 7·1	57

ε Eridani.

	− 3 1·5		38

o Eridani.

	− 1 29	− 5 17·4	38

υ Eridani.

	+ 1 6	− 4 50·5	56

53 Eridani.

7	+ 1 6·0	− 3 6·9	57

α Equulei.

10	+ 0 57	− 0 15·6	73

α Fornacis.

8	− 0$^{\text{m}}$ 9$^{\text{s}}$	−	56

α Geminorum.

7·5	− 8 33	− 2′ 30″·5	58
6·5	− 5 35	− 8 54·5	58
6	− 0 0·7	+ 0 3	58
8	+ 5 15	+ 8 37·8	58

γ Geminorum.

12	− 6 21·8	− 8 32·3	71
10	− 2 54·8		71
	− 2 47	+ 20 48·3	38
8·5	− 2 46·8	− 12 36·3	71
12	− 1 50·5	+ 1 32	39

δ Geminorum.

	− 0 43	+	40

η Geminorum.

7	− 2 36	+ 23 18	42

ι Geminorum.

10	− 1 7	− 22 20·3	52
tel.	+ 0 37·5	+	52
10	+ 0 51	− 0 7	52
10	+ 0 51·5	− 0 4	57

λ Geminorum.

	− 4 42	− 24 13·8	42
	− 4 41·8	− 24 16·2	58
12	− 0 44		58

μ Geminorum.

	+ 7 24	+ 3 54·3	51

ρ Geminorum.

8	0 0	+ 0 39	38

ζ Geminorum.

8	− 6 19	−	41
	− 0 1	+ 1 29	39

ψ Geminorum (Propus).

tel.	− 3 1		59
9	− 2 59·5	+ 1 9·7	42
7	− 2 59·5	+ 1 11·2	59
7·5	+ 5 37	− 7 32·5	59
9	+ 6 24	− 14 12·3	59
9	+ 7 23	− 14 12·1	59
10	+ 8 13	− 18 11	59

α Herculis.

6	− 11^{m}. 32^{s}	− 13′ 10″·8	65
9	− 2 38·5	+ 3 51·2	65
9·5	− 2 38·4	+ 6 49·8	65
9·5	− 2 38	+ 6 45·8	45
8	− 2 38	+ 10 31·1	65
9	− 2 27		65
9	− 2 21	+ 9 51·7	65
10	− 0 50	+ 11 25·2	65
9	− 0 19	+ 9 56·5	45
10	− 0 18·5	+ 9 47·4	65
7	+ 0 0·5	− 0 3	49
6·5	+ 0 0·75	− 0 3·8	65
9·5	+ 0 42		65
10	+ 1 56		65
9	+ 5 59·5	+ 44 26·7	65
12	+ 6 54·5	+ 2 19·4	65
12	+ 7 49	+ 33 23·3	65

θ Herculis.

8	− 7 18·8	− 8 38·6	68

ζ Herculis.

	+ 1 30	+	45

α Hydræ.

tel.	+ 1 19·5	+ 11 54·2	43
8	+ 5 24	+ 9 33·7	54

7 Lacertæ.

9	− 0 8	+ 4 58	50

α Leonis.

8	− 4 40	+ 8 34	44
tel.	− 0 10	+ 1 47	44
tel.	− 0 10	+ 1 44·3	59
tel.	+ 3 9·5	+ 5 13·4	59

β Leonis.

	− 3 25·8	− 19 13	44
8·5	− 0 30	− 17 37	44
tel.	− 0 11·5	−	44
	+ 0 55		44

γ Leonis.

	− 2 9	−	41
6	− 0 4	− 21 59·4	41

δ Leonis.

8	+ 0 5	+	43

ε Leonis.

7·5	− 0 35	− 17 49·6	60
8·5	+ 1 31·5	− 7 21·6	60

ζ Leonis.

7	− 0 16		61
7	− 0 5	+ 4 55·3	61

η Leonis.

6	− 0^{m} 6^{s}	+ 4′ 59″·5	76
6	+ 1 41	− 18 14·2	76

κ Leonis.

7	− 5 26	+ 2 54·6	58
8	− 0 30·5	− 15 54·8	58

ο Leonis.

7	+ 4 26	+ 2 49·8	57

π Leonis.

8	− 6 6	+ 0 48·3	44
7	− 2 6	+ 15 51·8	44

π_1 Leonis.

10	− 3 12	+ 2 30·7	61

π_2 Leonis.

9	− 4 50·5	− 2 34	61

ρ Leonis.

8	− 0 40	+ 6 56·8	61
7	− 5 10·8	+ 26 34·2	61
12	+ 6 21	− 4 32·2	61

ρ_2 Leonis.

10	− 2 16	−	41
6·5	+ 2 15	− 39 9·7	41

σ Leonis.

	+ 0 42	+	43

τ Leonis.

9	− 2 16·5	+ 26 50·2	60
8·5	− 1 4·5	+ 18 0·1	43
tel.	+ 0 1	−	43
10	+ 0 1·5	− 1 36	60

χ Leonis.

9	− 1 25·5	+ 14 34·3	59
	− 1 24·5	+ 14 36·3	43
8	− 0 23		59
	+ 1 4·5	+	43

α Libræ.

	− 0 11·1	+ 2 35·4	45

β Libræ.

tel.	− 3 5	− 5 38·9	63
9	− 3 5	− 5 45·5	45
8	+ 2 20	+ 7 52·9	63

δ Libræ.

	− 2 23·2	− 11 32·2	62
	+ 1 6	+ 10 2·9	62

l Libræ.

	+ 1^{m} 6^{s}	+ 10′ 2″·9	63

α Lyræ.

10	− 4 8	+ 7 7·6	48
9	− 3 59	+ 5 38·8	48
tel.	− 3 6·5	+	48
9	− 1 29	+ 8 11·2	48
10	+ 0 28·5		48
8	+ 1 30		48
8	+ 3 14·2	− 10 53·8	48
9	+ 7 7	− 16 18	48

β Lyræ.

8	− 4 11	− 8 46·4	46
	− 2 29		46
8	− 0 38	0 0	46
tel.	− 0 15	− 2 17·2	46
9·5	+ 0 2	+ 0 37·4	46

δ Lyræ.

tel.	− 3 12·5	− 3 40·5	65
tel.	− 1 13·5	− 1 12·5	65
	+ 0 2	− 0 41	65

ε Lyræ.

9·5	+ 0 0·2	+ 0 3	68
6	+ 0 2	− 3 33	68
6	+ 0 2·1	− 3 35·5	68

ζ Lyræ.

9	− 4 25	+ 47 11·4	67
8	+ 0 2·5	− 0 36·2	67

α Ophiuchi.

8	− 0 53	− 3 18	63
10	− 0 52	− 3 14·2	46
	+ 3 31	+ 7 58·9	63

β Ophiuchi.

8·5	− 1 34	− 10 51·4	63
10	− 2 46	− 23 30·4	63

γ Ophiuchi.

dupl. 8–8	− 3 20	− 5 56·5	64
	− 3 19	− 5 56	64

ζ Ophiuchi.

12	− 4 12	+ 1 33·7	63

A Ophiuchi.

8	+ 0 0	− 0 13·2	67
12	+ 0 18·5	+ 0 9	67

α Orionis.

10	− 5^{m} 39^{s}	+ 6′ 31″·8	41
9·5	− 2 52·5	− 5 25·8	58
8	− 1 41	+	41

β Orionis.

10	− 2 7	+ 7 38·9	58
tel.	− 1 34		58
	− 1 0·5	+ 2 55·2	40
7	− 0 59	+ 2 53·5	58

δ Orionis.

	− 2 23	+ 15 54·5	57
7·5	0 0	+ 0 50	41
10	+ 0 0·2	+ 0 56	57
	+ 2 27	+	42
	+ 2 55	+	42
	+ 3 8·3	+	42

ε Orionis.

8	− 2 42	+	38
tel.	+ 0 23	+	42

ζ Orionis.

10	− 0 49·2	+ 0 42·5	41

κ Orionis.

	− 0 47	− 10 24·4	59

α Pegasi.

	+ 0 48	− 14 53·4	53

β Pegasi.

	+ 1 22	− 3 0	54

ε Pegasi.

tel.	− 0 5·5	+	54

η Pegasi.

	− 1 16	− 54 37	54
tel.	+ 1 2·5	−	54

ζ Pegasi.

10	− 0 41	+ 15 8·8	74
tel.	+ 0 41	+	55
9·5	+ 0 41·5	+ 15 20	70
8	+ 2 15·5	+ 6 51·3	55
8	+ 2 15·5	+ 6 41·5	74
8	+ 3 54	+ 21 40·4	70

c Pegasi.

9·5	− 6 8·5	− 3 49	41
9	− 1 25	+ 3 0·5	78

α Persei.

	− 5m 8s	+ 12′ 39″·7	58
tel.	+ 1 45	− 6 33·6	58
	+ 1 45·5	− 6 32	55
8	+ 1 57		75
7	+ 2 26	− 5 57·4	75
10	+ 3 17	− 14 48·9	75
6	+ 4 3·2		55
7·5	+ 4 28	+ 0 17	58
	+ 4 28	+ 0 22·6	55
6·5	+ 5 1	− 19 53	75
8	+ 7 8	− 37 22·2	75

β Persei.

8	− 2 50	+ 9 12·4	75
8	− 2 49·7	+ 9 15·1	56
8	− 2 49·5	+ 9 9·5	38
9	+ 0 55·5	−	56
8·5	+ 3 10	+	56

δ Persei.

	+ 0 1	+	39
	+ 1 31·5	+ 2 12	39
	+ 2 30·8	− 6 57·7	57
	+ 2 42	−	39
7·5	+ 6 30	+ 13 26·9	57

ε Persei.

10	+ 2 18	+ 0 43·2	69
9·5	+ 2 18	+ 0 51	76
8	+ 3 21·5	+ 6 10·7	76
8	+ 3 53		76

θ Persei.

8	+ 4 39	− 2 1·6	72

α Piscis Austr.

7	− 6 16	+ 5 7·6	50
	+ 2 4	+ 8 42	50
7·5	+ 3 48·7	+ 44 50·2	50

ε Piscium.

12	− 4 58·7	− 17 2·3	70
9	− 4 58·5	− 17 0	72
10	− 1 45·2	+ 8 35·8	72
12	− 1 45	+ 8 45·7	70
	− 0 55	+	52
10	− 0 51·5	+ 3 0	76
	− 0 51	+ 2 56·4	52
8	− 0 51	+ 2 59·3	72
7	+ 3 38·5	+ 28 40	72

ζ Piscium.

7	+ 0 1·5	+ 0 10·5	73

κ Piscium.

	+ 0 19	− 8 13·6	52
7	+ 0 19·5	− 8 13·6	68
10	+ 3 45·5	− 33 49·9	68

λ Piscium.

7	+ 7m 21s	− 42′ 53″	76

π Piscium.

8	+ 0 32	− 6 31	73
	+ 1 17	+	56

ρ Piscium.

7	+ 0 25		56

ψ Piscium.

6	+ 0 1	− 0 30	74

ω Piscium.

6	− 4 8	+ 12 4	70
7·5	+ 2 6	− 4 43	70

λ Sagittarii.

	+ 0 12	+	49
	+ 1 21·5	+ 8 41	49

μ Sagittarii.

7	+ 1 28	+ 19 18	64
8	+ 1 30	+ 39 39·5	64
8	+ 2 42	+ 29 54	64

χ_1 Sagittarii.

9·5	+ 0 7	+ 5 35·6	64

χ_2 Sagittarii.

9	+ 0 49	+ 0 56	64

b Sagittarii.

8·5	− 2 40·2	− 6 40·3	66
8	+ 4 1	− 33 59	66

d Sagittarii.

8	+ 0 33	+ 4 39·2	66
8	+ 0 51·5		66
	+ 1 3		66
	+ 1 22	+	66

α Sagittæ.

8·5	+ 0 4·5	− 6 57·9	47
	+ 0 58	−	47
12	+ 2 43	− 3 36·0	47

α Scorpii.

8	+ 1 57	− 6 54·4	46

β Scorpii.

Neb. tel.	− 2 19	− 1 29·3	40
6	− 2 18	− 1 29·3	45
7	+ 0 0·7	+ 0 11·8	63
7	+ 0 1	+ 0 9·2	40
7·5	+ 0 1	+ 0 7·2	45
12	+ 0 18·5	+	63

δ Scorpii.

7	+ 1^{m} 52^{s}·5	−	45
12	+ 1 52·5	− 4′ 30″·9	62

λ Scorpii.

6·5	− 3 31	− 10 10·8	60

μ Scorpii.

8	+ 0 28	+ 1 37·5	65

ν Scorpii.

8	− 0 2	+ 0 33	45

β Serpentis.

	+ 0 14	+	42

γ Serpentis.

7	− 2 47·2	+ 20 55·5	63
	− 2 46·5	+	42
	+ 0 14		63

η Serpentis.

8		− 4 34	− 6 38	45
7	Dupl.	− 4 33·5	− 6 37·5	68
7		− 4 33	− 6 37·5	68

θ Serpentis.

	+ 0 1	− 0 5	50
9	+ 0 23	+	50

55 Sextantis.

6·5	− 3 59	+ 16 51·6	40
	+ 0 30	−	60

α Tauri.

8·5	− 11 28·5	+ 11 47·5	39
8	− 7 24·5	− 12 2·8	39
10	− 5 6	− 23 41·9	57
8	− 2 14·5	− 12 25·6	57
8·5	− 2 14·2	− 12 29	38

β Tauri.

8	− 2 56·1	+ 18 17·8	41

ε Tauri.

8	− 3 38	− 9 21	40

ι Tauri (præc.)

9·5	− 6^{m} 48^{s}·5	− 3′ 14″	41
tel.	+ 1 17	+ 18 44·8	41

Electra.

tel.	− 0 8	+ 0 32·5	38

γ Trianguli.

	− 0 37	+	53
	+ 0 12	− 4 34·2	53

α Virginis.

10	Dupl.	− 10 9	− 10 28	63
10		− 10 7	− 10 30	63
		− 7 46	. .	44
9		− 7 39·5	+ 37 27·4	63
7		− 4 48·5	− 8 10	44
7		− 4 48·5	− 8 6·2	63
8		− 3 34·6	− 8 4·8	63
		− 1 14·2	+ 21 38	44
8·5		− 1 14	+ 17 31·2	63

γ Virginis.

7	+ 1 50		60

η Virginis.

7·5	− 6 1	+ 1 40·3	42
	− 1 15	− 7 9·5	40

ζ Virginis.

7·5	− 0 26·5	+	43
	− 0 26	+ 20 48·5	62

ι Virginis.

7	+ 5 4	+ 43 31·4	61

τ Virginis.

	− 5 11	− 28 59·6	60

ξ Virginis.

9	− 0 44	+ 0 43·6	61
6	+ 2 39·5	− 0 47·9	61

b Virginis.

8	+ 1 14	− 1 15·6	61

EXTRACTS FROM TWO LETTERS FROM SOUTH AMERICA.

Lima, 27 Jan., 1883.

At Chorillos, near this, are staying M. Barnaud, Lieutenant de Vaisseau and M. Favreau, Enseigne de Vaisseau, members of the French Venus Expeditions to Chili. Chorillos is the landing point of the cable from Valparaiso and Panama. The French astronomers in conjunction with two colleagues now at Valparaiso are determining the difference of longitude; they have 2-inch Transit Instruments with chronographs and chronometers, and the cable is led directly into the Observatory. The instruments are similar at both stations, the observers do not interchange stations, but the personal equation has been determined for wire transits and signals transmitted by Thomson's galvanometer. The strength of current is adjusted by a Rheostat to a constant strength. A triangulation will connect Chorillos, Callao and Lima, distant some six or seven miles from each other. The connection of Valparaiso with Buenos Aires on the one hand, and with Callao and Panama on the other will complete the circuit of the greater part of South America, the chain from Greenwich to Buenos Aires through Lisbon, Madeira, St. Vincent, Pernambuco, Bahia, Rio Janeiro amd Monte Video having been finished by Lieutenant Commander Green, U.S.N., in 1879. It is deeply to be regretted that a spirit of undue economy prevents the British Government from taking part in these important operations, which are so closely connected with navigation and geography on a large scale.

RALPH COPELAND.

La Paz, Bolivia, 21 Feb., 1883.
(12050 ft. ± above sea level.)

For the first time for ten days the sky is tolerably clear and remarkably dark, although the Moon is almost exactly full and at an altitude of some 30°. At $9^h\ 30^m$ local mean time, to test the clearness of the air and the visibility of 5th and 6th magnitude stars, I made a naked eye sketch of the Hyades and Pleiades which were also roughly at the same altitude as the Moon, but considerably more than 90° distant from her. In the Pleiades I distinctly made out ten stars, D.M. + 24°, 553 and 556, both of 7·0 mag., being seen as one star, and D.M. + 24°,546, of 6·3 mag. being clearly visible. In the head of Taurus I made out seventeen stars, two of which, D.M. + 16°,586 and + 16°,605 (of 6·0 and 5·0 mag.) are not in Argelander's *Uranometria Nova;* I also saw σ Tauri plainly double.

As it is now near the close of the rainy season, I hope shortly to be in a position to report something of what can be done with a six-inch Refractor at 14360 feet above the sea level. My station is at Vincocaya, between Arequipa and Puno. In the mean time I am endeavouring to obtain the height of the Ilimani.

RALPH COPELAND.

THE GOVERNMENT EXPEDITION TO MADAGASCAR TO OBSERVE THE TRANSIT OF VENUS, ON DECEMBER 6th, 1882.

BY REV. S. J. PERRY, F.R.S.

It would certainly be premature, and it might also show a want of courtesy, to express at present any opinion as to the probable alteration of the limits of the solar parallax due to the observations of the late Transit of Venus, but it may not be uninteresting to publish a few details concerning the manner in which the official plan of operations was practically carried out in the Expeditions of 1882. For obvious reasons I shall confine my remarks to the Expedition, of which I was myself in charge, which was sent to the South West Coast of Madagascar.

The Rev. W. Sidgreaves, S.J., who accompanied me, had previously observed the Transit of Venus in 1874 on the Island of Desolation, and our assistant, Mr. W. Carlisle, has been for many years attached to the Stonyhurst Observatory. A month was spent by all three at the Radcliffe Observatory, mounting and dismounting the instruments we were to take with us, and comparing the results of our observations with those of other astronomers.

On August the 29th, we sailed from London in the R.M.S. *Kinfaun's Castle* for the Cape of Good Hope. We were provided with two 6-inch Equatoreals, one by Simms, and the other the Corbett achromatic mounted on an Equatoreal stand made by Dallmeyer for the 4-inch photoheliographs of 1874. Knowing that valuable results might be obtained with a 4-inch Equatoreal, I also took with me the Jones instrument from the Stonyhurst Observatory. This Equatoreal had formerly been used by Captain Maclear, R.N., for spectroscopic work in Spain during the total solar eclipse of 1870, and also by Captain Fairfax, R.N., for observing Venus on Desolation Island in 1874. The altazimuth, on which our time was to depend, and also our latitude, was already at the Cape of Good Hope, and we received it on our arrival in excellent working order from the hands of Dr. Gill, Astronomer Royal of Cape Town Observatory. The huts for the three principal instruments formed part of our luggage from London, but we trusted to the Cape for a supply of tents for dwelling on shore at Madagascar. The longitude of our station was to be determined by a chronometer run from the Cape, touching if possible at Durban to secure a good sea-rate, so we borrowed seven chronometers from the Royal Observatory at Greenwich. which, together with an equal number belonging to H.M.S. *Fawn,* were thought to be sufficient to secure reliable results. An excellent sidereal clock by Dent completed our astronomical equipment.

Thinking it advisable to be prepared for other scientific work, I was kindly supplied with a set of magnetic instruments by Mr. Whipple of Kew, and Mr.

Carlisle took with him all that was necessary for collecting and preserving any natural history specimens we were likely to meet with in our travels. A small camera with a good supply of Newman's dry plates was found to be a very interesting addition to our travelling gear, as it enabled us to obtain numerous photographs of natives, and furnished at the same time an accurate and permanent record of our observing station and of its vicinity. In 1874 we purchased in London a large supply of provisions for a six months' stay on a desert island, but on the present occasion we left the whole of this care in the hands of Captain Aldrich, R.N., of H.M.S. *Fawn*, and we had no cause to regret the change, as every attention was paid to our personal comfort, as well as to the efficiency of our work.

Our journey from London to the Cape of Good Hope was uneventful, with the sole exception of a display of phosphorescence near the equator, which surpassed in splendour any thing witnessed in previous voyages. I did not see myself the grandest part of the display, so I will quote from a description given in the "Month" by the Rev. W. Sidgreaves. "We had noticed a bright line of light on the horizon before the ship, extending many degrees of the circle on both sides. In about fifteen minutes the ship was in the midst of what had seemed a mere line, and we found ourselves surrounded by a sea of large and most brilliant stars. It would not be correct to call it a sea of fire. The appearance was not that of fire but of a cold starry light, which shone with doubled brilliancy from the contrast of the deep darkness of the intervening spaces. No friction seemed needed to produce the light; for the sea was calm, and had scarcely a broken wave out of the ship's track. Thick clusters of these stars were as numerous in all directions as the spaces of single brilliants. And where the ship had lashed them up together, there appeared a foaming rushing light which lit up the sides of the ship as if by a powerful electrical machine. In a short half-hour we had passed through the silvery sea, and all became as remarkably dark as the brightness had been surprising."

Arrived at the Cape of Good Hope our scientific expedition was near being brought to an abrupt termination by a large iron steamer, which, with fires extinguished, was rapidly approaching us midships under the guidance of a tug, when their hawser suddenly snapped, and the huge wedge of iron bore straight down upon us as we lay at anchor outside the harbour. Most fortunately for us several waves in quick succession struck the helpless vessel, and drove her past our bows, though she passed within a yard of our ship.

Our first danger past, we were soon involved in another, which threatened to be more disastrous. The small-pox was raging among the Malays at Cape Town, and the Admiral at the naval station at Simon's Bay wisely refused to allow any of his men-of-war to touch at the infected port. This might seriously have impeded progress as we were to be conveyed to Madagascar in H.M.S. *Fawn* then at

Simon's Bay. Personally we escaped the epidemic, thanks to a most generous offer of hospitality from Dr. and Mrs. Gill, and the hearty cooperation of the Astronomer Royal soon smoothed all difficulties and enabled us to convey our heavy and delicate luggage safely overland from Cape Town to Simon's Bay.

In the mean time Captain Aldrich, R.N., had been steadily at work preparing the *Fawn* for sea, and had done all in his power to combine the most efficient carrying out of our scientific plans with the greatest comfort of the observers. H.M S. *Fawn* is a surveying ship with a spacious chart-room, and this was partitioned into roomy apartments for two of the observers, whilst the Captain kindly shared his own private cabin with the third. Three large square tents with wooden floors were taken on board for the use of the observers, and several barrels of cement were shipped for making concrete piers for the instruments. The chronometers of the *Fawn,* which were to be used for the longitude run from the Cape to Natal and Madagascar, were placed side by side with our own in an excellent berth in the wardroom. Telegraphic signals were then sent from the Royal Observatory to Simon's Bay to rate the chronometers whilst on board, and the time was later on to be sent daily from Cape Town to the Natal Observatory during our exchange of signals at Durban.

A six days' sail brought us from Simon's Bay to Durban, but here again we were face to face with our old adversary, the small-pox. We came from a port in which the epidemic had shown itself, and therefore our fate was quarantine of the severest type. Not allowed to land, with the yellow flag adorning our main-mast, and correspondence even by fumigated letter most rigidly forbidden, we had little hopes for a time of obtaining any satisfactory signals for longitude. After a day or two of irritating delay we were finally allowed to send a disinfected note to the Astronomer, Mr. Pett, and thus enabled to arrange an exchange of time signals with the Natal Observatory. Guns, rockets, and sun flashes were all tried, but the most satisfactory for a long distance were the big guns, fired at minute intervals by Captain Aldrich, the sound being timed by the astronomers on board, and the flash by those on shore. As soon as our signals proved to be thoroughly reliable, we bid a hasty adieu to terror-stricken Durban, where we had been for five days exposed at anchor to the full force of the great rollers of the Indian Ocean.

From Durban a fair wind soon brought us into the Mozambique Channel, and our journey was quickly approaching its termination, when a fire broke out on board the *Fawn.* It was over in a few minutes, but the whole affair might easily have become serious, and it afforded the Captain a good test of the efficiency of his fire drill.

On the evening of the same day, the 22nd of October, we cast anchor between the small island of Nos Vey and Madagascar. Nos Vey is a heap of sand, three quarters of a mile in length by one quarter in breadth, surrounded by a coral reef, and inhabited by French and English traders with about sixty native families from the mainland. This large bank of sand was uninhabited a few years since, when Captain Macé, the chief trader from Bourbon, finding it impossible to live on good terms with the natives on account of their thieving propensities, took possession of the desert spot, fortified it, and has since been enabled by courage and tact to start a thriving trade in the orseille plant, tortoises, timber, &c. He maintains small trading stations among the tribes of the S.W. coast, but never sends a large quantity of stores to any place at a distance, so that any possible robbery will be but of little moment. Small schooners connect these less important stations with Nos Vey, where the principal stores are kept. The difficulty of crossing in large numbers from Madagascar to Nos Vey in native canoes, and the total absence of water and food on the island, render any considerable landing of natives, and consequently any sudden attack in force, almost impossible. The agents of an English firm have followed the example of Captain Macé, and now two Englishmen and a few Norwegians are also settled at Nos Vey, as well as several smaller French and Bourbon traders.

A few weeks before our arrival two Americans, Messrs. Emerson and Hulett, who were travelling near Tullear, had unfortunately allowed the natives to become aware of their being in possession of a quantity of gold, and were in consequence treacherously attacked. Emerson was shot dead along with a native follower, and a French attendant was not only slain, but his limbs were nailed to a tree, in punishment, it is supposed, of some previous misconduct. Hullet and another servant were wounded in both arm and leg, but by hiding in the woods they both managed to escape with their lives. They were afterwards brought from Tullear to Nos Vey to have their wounds dressed by Dr. Sweetman, R.N., of H.M.S. *Fawn*, who most kindly attended all the sick in the neighbourhood of our station.

The insecurity on the main island made us choose Nos Vey for our astronomical work, and we received a most friendly welcome from the French and English traders. Our daily supply of water, and the cattle we required, were brought over from Madagascar by Mr. Oman, the agent of the English firm; the natives themselves would not supply us directly with provisions, as they were afraid of us, thinking we had arrived for the purpose of punishing the perpetrators of the late outrages.

Having landed with Captain Aldrich to make inquiries and to inspect the position, we chose the south end of the island for our station, and made arrangements with a native chief to prevent the encroachments of the neighbouring villagers. A flag-staff was erected near our tents, and a sailor stationed there on duty day and night. To secure watchfulness a lamp was hoisted as the clock struck each hour throughout the night. On questioning the chief as to how the natives were to be warned off the line drawn in front of our encampment: "Shoot them," was his laconic reply. Whether the Malagasy heard of this striking advice, or whether they believed in the report that the holes we were digging for our instruments were a preparation for blowing up the island, they suddenly disappeared from our neighbourhood in a single night, carrying with them the reed walls of their huts, and entirely dismantling the village of Nos Vey.

The coral reef surrounding our island of sand made it necessary to land the heavy goods near the time of high water, and this delayed us a little; but thanks to the energy of Lieutenant Marx we soon succeeded in getting all our instruments safely on shore, and the only accident was the capsizing of a large boat full of men at the edge of the reef as they were returning to the ship.

In the mean time a party of blue-jackets under Lieutenants Keary and Maresco, were hard at work under a burning sun digging holes in the sand ten feet deep, and filling them partly with heavy pieces of coral and partly with concrete. The whole island consisted of loose sand, so we were forced to line the holes for the foundations with planks, but the result was very satisfactory, and provided solid supports for our Altazimuth and Equatoreals, and also for our magnetic pier.

A coarse grass overspread a great portion of the island, so the equatoreals were placed to the east of the thickest part of this vegetation in order to diminish the glare from the sand at the time of the Transit of Venus. This grass which was destined to give us important aid on the hot evening of the Transit, was nearly frustrating all our hopes. At an early hour on the 6th of November a man carelessly threw on the sand a burning match with which he had just lit his morning pipe, and in an instant the dry grass was in a blaze, spreading with great rapidity. Fortunately the wind was from the East, and a large body of officers and men, who landed at once from the ship, were finally able with the assistance of native servants to check the fury of the flames. Had the direction of the wind been N.W., our tents, huts, and instruments would all probably have shared a common fate, and we should have had the misfortune of returning empty handed; our Expedition would have been a complete failure.

The altazimuth instrument was placed at the S.E. corner of the island, so as to secure a commanding horizon E. and W. for time observations, and N. and S. for latitude stars, with a good view of the opposite coast in order that it might serve as a base station for the survey. This instrument was unfortunately rather too near the shore, and the noise of the tide, added to that of the wind, not unfrequently made it impossible to hear the beats of the sidereal clock at the requisite distance.

Our longitude was to be made dependent in the first place on a double run with a dozen chronometers from the Cape and Durban to Madagascar, but the importance of the result made us anxious to check the chronometer run by lunar observations. We therefore entered into an agreement with the Astronomer Royal at the Cape, and with Colonel Tupman, R.M.A., the Chief of the New Zealand Transit Expedition, to observe the occultation of all stars down to the 9th magnitude during the first half of each lunation. On one night I was fortunate enough to time accurately seventeen occultations, and on several other nights I obtained a fair number of observations. Of course few of these stars are to be found in the Nautical Almanack, still most of those that have been observed at two stations will, it is hoped, be afterwards identified if necessary. We also took some lunar altitudes with moon culminators for longitude.

Whilst Father Sidgreaves and myself were engaged in these observations, Captain Aldrich, assisted by Lieutenant Helby, R.N., the navigating officer of the *Fawn*, was taking double altitudes of the sun and of stars for time, and observing stars for latitude; and the results they obtained prove that the sextant, mounted on a solid stand, with a mercury horizon, can, in experienced hands, become a thoroughly reliable instrument. I mention this because I have heard the sextant condemned as an instrument for travellers by very high authority, and I saw it at Nos Vey practically holding its own against an excellent full-sized altazimuth. Captain Aldrich was also personally engaged in the erection of the 4-inch Equatoreal that had been brought for his use from Stonyhurst Observatory, and in providing adequate protection for it against the strong South winds.

As soon as the heavy work of digging foundations, mixing concrete, landing goods, and erecting instruments was completed, a survey of the neighbouring coast was immediately commenced, the Lieutenants of the *Fawn* going away with their men in boats to take soundings and measure angles, and Captain Aldrich exchanging flashes with them from our Altazimuth hut. The flags, which soon appeared on prominent points of the shore and reef, gave some umbrage to the natives, especially as one flag was said to have been planted above the tomb of an ancient King. One of the present King's sons threatened to pull down all the flags, but he was fortunately dissuaded by his followers, who have a salutary dread of the firearms of Europeans.

Terrestrial magnetism likewise engaged our attention, and as soon as Mr. Carlisle could be spared from work connected with Astronomy, he commenced a series of observations of the Dip, Declination and Horizontal Force of the Earth's magnetism. He also collected all the specimens of Natural History that could be met with on so small an island. Time did not allow us to pay more than one hurried visit to Madagascar itself, when we spent a few hours in the town of Tullear, near the spot where the murders had been committed a few weeks before our arrival.

The weather at the end of October and at the beginning of November was everything we could desire, except perhaps that the South wind was rather boisterous, and forced us to erect a special reed wall to protect one of the Equatoreals, as the instrument vibrated very considerably when protected only by its hut. The fine sand raised by the wind was also a little troublesome, but less so than it would have been on the opposite shore. A fortnight before the Transit everything was in good working order, and we began to feel certain that results must be favourable,.

when the general direction of the wind changed, and we soon had rain and cloud with the Northerly breezes. This made us anxious, and we had little encouragement even up to the very morning of December the 6th, for the sky was then completely covered with dense clouds. Most happily for us the wind veered round from North to South in the course of the morning, and as it freshened, the sky cleared, and before noon we knew well that our adversaries would be strong wind and sand in lieu of cloud and rain. Extra protections were therefore erected for the Equatoreals a little to the West of South, the natives and sailors working together under the direction of Captain Aldrich and the English traders, as no time was to be lost.

The crew of the *Fawn* were most anxious for the complete success of the Expedition, and all had shared our uneasiness about the weather. But some of the men also expressed considerable doubts on account of the distance of Venus from the Sun, for in spite of the Nautical Almanack they felt certain, when they saw the great altitude of the planet early in the evening towards the end of November, that "she could never fetch up in time."

As the hour of contact approached the sky became perfectly clear, the wind increased in strength, and the fine sand fell in showers over our instruments. We compared our chronometers with the standard sidereal clock, and focussed our telescopes on the mottled surface of the Sun, which showed out splendidly. The 6-inch Equatoreals were both provided with solar prisms and with excellent neutral wedges, and fortunately I had procured a similar but smaller wedge for my theodolite, which could be adapted to the 4-inch Equatoreal of Captain Aldrich, but this instrument had no solar prism. In spite of the misgivings of the bluejackets, Venus was up to time, and when she had passed about two-thirds of the way across the solar limb the bright ring, formed in part at least by the planetary atmosphere, began to show itself. The portions of the ring nearest the solar disk were the first to become visible, but the light soon shone all round that part of the limb of Venus which was still outside the sun. No atmosphere was apparent around that portion of the planet which had already entered upon the sun's disk. The principal phases at internal contact, viz., the diminution of intensity in the ligament, and its final rupture, were well seen by all three observers, and no doubt can be entertained as to the nature of the phenomena observed; but the changes were not instantaneous, and could not be timed with anything approaching to the accuracy of model practice. Chronometers were again compared immediately after the observation of internal contact, and, the sky continuing clear, we were able the same night to secure very satisfactory time observations with the altazimuth. Clouds however again began to show themselves on the following day, though the nights of the 7th and 8th were occasionally clear enough for securing East and West stars for time. By instructions we were required to take, if possible, time observations on three nights before and three immediately after the Transit, and the breaking up of the fine weather made us naturally anxious to make the same observations serve for our chronometer run back to Natal. Many signals were exchanged between the longitude chronometers on board and the standard sidereal clock on shore by means of gun flashes, which gave excellent results, the guns being fired at night about the middle of the star observations.

Whilst this work was being carried on at the altazimuth, a strong party of engineers and blue-jackets were expending their energies on dismounting and packing the Equatoreals and conveying them when ready to the ship. On the morning of the 8th the tents were taken down and everything removed to the *Fawn* before sunset, except the altazimuth and its sidereal clock. Stars were again observed East and West for time, and at 10 P.M., when the sky clouded in hopelessly, we compared chronometers with standard clock by gun flashes, dismounted the clock, soldered down the delicate parts, placed the altazimuth carefully in its boxes. and at 2 A M. were on our way to the ship for the last time, taking all the remaining instruments on board. Our huts we had previously sold to one of the Norwegian traders.

We set sail at daybreak, having on board Captain Macé, whose trading vessel had become unseaworthy, Mr. Hulett, the American wounded in the affray at Tullear, and a European who wished to return to Natal. On the 14th of December we again cast anchor in the open roadstead of Durban. This time we were clear of all quarantine regulations, and the bar at the mouth of the harbour was the only hindrance to landing and holding communications with astronomers on shore.

During our absence in Madagascar the Director of the new Observatory at Durban, Mr. Neison, so well known from his lunar calculations, had arrived from England and obtained good observations of the Transit of Venus. His Observatory was still unprovided with a standard sidereal clock, so I was fortunately able to supply this urgent need by leaving our excellent Dent instrument in his hands. The new Observatory is well situated on the hill commanding Durban, and the colonial authorities are very generously disposed to provide all that is requisite to make the establishment in every way complete. The astronomical buildings are already finished, and five acres of land have been granted to the observer to use at his discretion. At present the woods approach too near the Observatory, and considerable anxiety is felt about leaving windows open, as the monkeys in the wood are numerous and very inquisitive, and might take a fancy to small eye-pieces and other portable parts of the instruments. The woods will therefore shortly be cleared from the immediate neighbourhood of the Observatory, and the grounds laid out as soon as funds have been procured for building the dwelling-house of the Government Astronomer. A well-mounted 8-inch Equatoreal by Grubb is already in position, and a telegraph wire laid down to the Post Office in Durban; but the small-pox still detained the Transit Instrument at the Cape Observatory up to the time of our visit, and the mean time clock had not yet arrived at its destination. The Observatory was thus still left dependent for its time on two chronometers, and on telegraphic signals from the Cape of Good Hope. When all the instruments are in good working order, it is the intention of the Director to devote his energies principally to lunar observations.

Thanks to the zealous cooperation of Mr. Neison we soon got reliable time signals for longitude, for the big guns of H.M.S. *Fawn* were again brought into requisition with the happiest result. From Durban we despatched telegrams to England, and also sent copies of our observations by the Royal Mail for greater security. Our journey from Durban to the Cape was at first pleasant enough, but as we approached Cape Agulhas a head wind developed into a storm, and we had a lively time of it for three days, finding ourselves some thirty miles further from our destination at the end than we had been at the beginning of the storm. At 8.30 P.M., on Christmas Day we cast anchor in Simon's Bay, and soon forgot our troubles in the comfortable home provided for us at the Royal Observatory by Dr. and Mrs. Gill. Leaving our large theodolite at the Observatory, our seven chronometers on board H.M.S. *Fawn*, and our original observations in the hands of the Government agent at Cape Town, we returned to England with our remaining instruments in the R.M.S. *Drummond Castle*, passing quietly up the Atlantic, but receiving a good tossing in the Bay of Biscay, and a boisterous welcome to our native shores by a gale in the Channel and a half hurricane at the mouth of the Thames.

OBSERVATION OF THE TRANSIT OF VENUS, DECEMBER 6, 1882, AT THE OBSERVATORY OF QUEEN'S COLLEGE, CORK.

By Professor J. ENGLAND.

The day proved very favourable, as the sky was almost cloudless for the whole forenoon and until about 3 o'clock, when the Sun was for some time obscured by clouds.

The instrument used was the 8-inch Refractor by Grubb, furnished with a polarizing eyepiece, in which the field of view was contracted to a diameter of about 5′. Magnifying Power used was about 90. The Transit Instrument not being as yet erected, the time was taken by a chronometer regulated to Greenwich Mean Time and compared with the time gun at one o'clock on the day of the Transit.

The eyepiece was carefully focussed on the East limb of the Sun, but as the limb was *boiling* considerably during the time of Ingress, the exact time of external contact was not noted; however, at $2^h\ 2^m\ 40^s$ a sensible notch was observed on the Sun's limb. It gradually increased, and for the whole time of Ingress the limb of the planet on the Sun's disc was very sharply defined. Some minutes before second contact the whole of the planet's disc became visible, about two-thirds of the diameter being on the Sun. The entire limb was well defined, but a faint although perfectly sensible light was noticed on the Eastern limb of Venus, reaching to the cusps, and midway between them about $\frac{1}{12}$ of the diameter in breadth. At $2^h\ 21^m\ 51^s$ the cusps nearly joined but were still separated by the dark disc of the planet. At $2^h\ 22^m\ 11^s$ the prolongation of the Sun's limb appeared to be tangential to that of Venus, and at $2^h\ 22^m\ 17^s$ the Sun's limb appeared to the East of the planet. From this time until the Sun was obscured by clouds, Venus appeared as a well-defined black circle on the Sun's disc.

The position of the Observatory as given by the Ordnance Map is: Lat. $+51°\ 53'\ 30''$, Long. $0^h\ 33^m\ 58^s$ West.

BEOBACHTUNG DER PARTIELLEN SONNENFINSTERNISS ZU O'GYALLA, 1882 MAI 16.

Herr Major v. Reviczky beobachtete während der Finsterniss die Ein- und Austritte der grösseren Sonnenflecken in die dunkle Mondscheibe, sowie die beiden Berührungen von Sonne und Mond. Er benutzte zu diesen Beobachtungen einen 4-füssigen Cometensucher. Für den zweiten Contact liegt ausserdem noch eine von mir am Heliometer erhaltene Beobachtung vor. Aus den notirten 14 einzelnen Berührungen des Mondrandes mit den Kernen der Flecken leitete ich folgende Zeiten für den Eintritt der Mitte der Flecken in den Mond ab—

Erster Contact	19^h	8^m	$19^s\cdot4$	M.Z. O'G. =	18^h	4^m	55^s	M.Z. Paris.
Fleck 1		35	53·3			32	29	
2		37	40·1			34	15	
3		40	33·7			37	9	
4		42	59·6			39	35	
5	20	18	37·4		19	15	13	
1		20	47·1			17	22·5	
4		27	23·3			23	59	
Zweiter Contact		58	3·9			54	39	
		57	29·9			54	5	

Der Fleck 5 wurde nicht völlig vom Monde bedeckt.

Es wurden nun nach den Angaben der Connaissance des temps die parallactischen Oerter von Sonne und Mond für jede volle Stunde von Mai 16, 16^h bis 22^h M.Z. Paris berechnet. Dabei wurden die Newcomb'schen Correctionen der Hansen'schen Tafeln berücksichtigt. Der Sonnenhalbmesser wurde nach Heliometermessungen zu $359''\cdot92$ angenommen; für den Mondhalbmesser dagegen der Tafelwerth beibehalten. Aus den erhaltenen Oertern wurde der Rectascensions und Declinationsunterschied der Mittelpunkte von Sonne und Mond in Reihen entwickelt. Bezeichnet man mit A, D und R, Rectascension, Declination und Radius der Sonne, mit a, δ und r dieselben Grössen für den Mond, und wählt man ferner als Einheit für t die Stunde und setzt $t = 0$ für 19^h M.Z. Paris, so wird—

$$\begin{array}{rrr}
A - a = -402''\cdot15 - t\;1693''\cdot170 & D - \delta = +1105''\cdot2 - t\;440''\cdot11 & D = +19^\circ\,19'\,6''\cdot2 + t\;34''\cdot25 \\
+ t^2\;\;68\cdot432 & + t^2\;\;15\cdot86 & R = \;\;948''\cdot8 \qquad\qquad\qquad \\
- t^3\;\;3\cdot419 & + t^3\;\;2\cdot42 & r = \;\;963''\cdot87 \qquad + t\;1''\cdot859 \\
- t^4\;\;0\cdot408 & - t^4\;\;0\cdot23 & - t^2\;0\cdot171 \\
+ t^5\;\;0\cdot014 & - t^5\;\;0\cdot06 & - t^3\;0\cdot030 \\
+ t^6\;\;0\cdot007 & - t^6\;\;0\cdot02 & + t^4\;0\cdot001
\end{array}$$

Vermittelst dieser Relationen wurden nun nach bekannten Formeln die Entfernung Δ der Mittelpunkte beider Gestirne, ihr Positionswinkel P und die einer Aenderung des Mondortes entsprechende Aenderung der Entfernung für die verschiedenen Beobachtungsmomente berechnet. Es ergab sich—

18^h	4^m	55^s	$\Delta = 1902''\cdot7$	$P = 217^\circ$	$0'$	$13''\cdot8$	$d\Delta = -0\cdot568\,da$	$-0\cdot751\,d\delta$
	32	29	1360·8	195	41	12·9	− 0·255	− 0·963
	34	15	1335·4	143	49	31·4	− 0·226	− 0·971
	37	9	1297·1	190	36	45·1	− 0·174	− 0·983
	39	35	1268·5	187	47	54·1	− 0·128	− 0·991
19	15	13	1264·8	141	48	46·7	+ 0·583	− 0·787
	27	22·5	1288·5	139	25	4·0	+ 0·614	− 0·760
	23	59	1373·4	132	41	3·6	+ 0·694	− 0·679
	54	39	1934·4	111	51	32·5	+ 0·876	− 0·375
	54	5	1912·9	112	7	17·8	+ 0·874	− 0·379

Für die benutzten Sonnenflecken leitete ich aus den hiesigen und den von Professor Spörer mir gütigst zur Verfügung gestellten Potsdamer Beobachtungen folgende heliocentrische Oerter ab—

Fleck 1	Mai 15·678 M.Z.P.	$l = 58^\circ\cdot08\;(\pm\;0^\circ\cdot060) - 0\cdot6574\,t$	$b = -\;28^\circ\cdot31$
2	16·774	58·06	− 26·02
3	18·890	$45\cdot86\;(\pm\;0\cdot139) - 0\cdot5963\,t$	− 30·77
4	18·031	$44\cdot71\;(\pm\;0\cdot126) - 0\cdot5478\,t$	− 29·56
5	18·667	$17\cdot20\;(\pm\;0\cdot071) - 0\cdot0135\,t$	− 16·33

Aus diesen Positionen wurden nun rückwärts der geocentrische Abstand ρ und der Positionswinkel p der Flecke für die verschiedenen Beobachtungszeiten berechnet. Unter Zuhülfenahme des Ephemeridenwerthes P ergab sich dann der beobachtete Werth von Δ und ausserdem die einem Fehler in den Radien entsprechende Aenderung desselben. Die numerische Rechnung führt zu folgendem Resultate—

$18^h\ 32^m\ 29^s$	Fleck 1	$\rho = 431''\cdot0$	$p = 173^\circ\cdot96$	$\Delta = 1361''\cdot2$	$d\Delta = +\ 0\cdot423\ dR$	$+\ 0\cdot986\ dr$
34 15	2	401·3	176·93	1339·9	+ 0·405	+ 0·993
37 9	3	456·2	154·55	1293·7	+ 0·389	+ 0·961
39 35	4	441·8	151·11	1280·6	+ 0·374	+ 0·963
19 15 13	5	544·6	95·67	1258·1	+ 0·398	+ 0·892
17 22·5	1	434·1	174·68	1285·7	+ 0·374	+ 0·996
23 59	4	441·0	151·75	1370·4	+ 0·440	+ 0·989

Ferner hat man—

$18^h\ 4^m\ 55^s$	$R + r = 1910''\cdot8$
18 54 39 18 54 5	1914·2

Die Vergleichung mit den aus der Ephemeride folgenden Werthen von Δ führt zu folgenden Bedingungsgleichungen zwischen den eingeführten Correctionen des Mondsortes und der Radien—

$+\ 0\cdot568\ da$	$+\ 0\cdot751\ d\delta$	$+\ 1\cdot000\ dR$	$+\ 1\cdot000\ dr$	$+\ 8''\cdot1 = 0$
+ 0·255	+ 0·963	+ 0·423	+ 0·986	+ 0·4 = 0
+ 0·226	+ 0·971	+ 0·405	+ 0·993	+ 4·5 = 0
+ 0·174	+ 0·983	+ 0·389	+ 0·961	− 3·4 = 0
+ 0·128	+ 0·991	+ 0·374	+ 0·963	+ 12·1 = 0
− 0·583	+ 0·787	+ 0·398	+ 0·892	− 6·7 = 0
− 0·614	+ 0·760	+ 0·374	+ 0·996	− 2·8 = 0
− 0·694	+ 0·679	+ 0·440	+ 0·989	− 3·0 = 0
− 0·876	+ 0·375	+ 1·000	+ 1·000	− 20·2 = 0
− 0·874	+ 0·379	+ 1·000	+ 1·000	+ 1·3 = 0

Zur Bestimmung der Unbekannten liefert die Methode der kleinsten Quadrate folgende Normalgleichungen—

$$
\begin{array}{rrrrl}
+\ 3\cdot21516\ da & -\ 0\cdot86696\ d\delta & -\ 1\cdot63408\ dR & -\ 2\cdot23362\ dr & = -\ 1\cdot53308 \\
 & +\ 6\cdot09108\ d\delta & +\ 3\cdot51422\ dR & +\ 6\cdot84591\ dr & = -\ 0\cdot55998 \\
 & & +\ 1\cdot26803\ dR & +\ 0\cdot63106\ dr & = +\ 0\cdot07114 \\
 & & & +\ 0\cdot01496\ dr & = +\ 0\cdot01069
\end{array}
$$

Die Fehlereinheit ist 20″·2. Die Gleichungen können offenbar zu einer Bestimmung von $d\ R$ und $d\ r$ nicht benutzt werden, man muss sich vielmehr darauf beschränken die Correctionen der Coordinaten als abhängig von den Fehlern der Radien darzustellen. Dies geschieht durch folgende Auflösung—

$$
\begin{array}{rl}
da = & -\ 10''\cdot13 + 0\cdot354\ dR + 0\cdot392\ dr \\
d\delta = & -\ 1''\cdot86 - 0\cdot577\ dR - 1\cdot124\ dr
\end{array}
$$

Die Annahme $dR = dr = 0$ vermindert die ursprüngliche Summe der Fehlerquadrate 715″·45 auf 396″·10, während das durch vier Unbekannte zu erreichende Minimum = 391″·6 ist. Für den wahrscheinlichen Fehler der Unbekannten finde ich $r\ (da) = \pm\ 2''\cdot69$ und $r\ (d\delta) = \pm\ 3''\cdot10$.

Dr. H. Kobold.

O'Gyalla, 1883, April 21.

RECENT LITERATURE.

Monograph of the central Parts of the Nebula of Orion. By Edward S. Holden. (Washington Observations for 1878, App. I., 230 pp. 4to.) One of the chief characteristics of modern scientific researches is a strong desire to discuss to the utmost the results deduced by previous workers in the same field, and to found the final conclusions as much on them as on the new investigations. In Professor Holden's memoir we find accordingly a most complete resumé of all published observations and drawings of the central parts of the Nebula of Orion, even referring to those made with small telescopes, or in a cursory manner (the only omission we have noticed being that of a few observations made at Birr Castle after 1867), and accompanying these extracts are copies of the original drawings in woodcuts on a uniform scale. These woodcuts can of course in most cases not compete with the original drawings or engravings, but they are chiefly intended to make the text comprehensible and have never been used as evidence. Beginning with Huyghens (1656 and 1694), Professor Holden examines and compares all the early representations of the nebula, which prove the general features of the object to have remained unchanged during two hundred years. He has been able to examine the original observations by W. Herschel in the archives of the Royal Society, and concludes that the changes which Herschel in 1811 thought he had found the nebula to have undergone since 1774, did not take place. We are glad to see Schröter's observations so carefully discussed, partly because he was the first to point out several minute but interesting details, partly because we believe that anything emanating from this indefatigable observer is worthy of attention. Of more modern observations we shall particularly mention those of Miss Caroline Lassell, part of whose large unpublished drawing, made in 1862 with the four-foot reflector at Malta, is reproduced in a woodcut. The observations of d'Arrest have hitherto been little accessible, being published in a Danish paper on the spectra of nebulæ (1872), but they are here translated (slightly condensed) and must be considered as a very valuable feature in Professor Holden's memoir.

The Author's own observations were all made with the 26-inch refractor at Washington, in the years 1874 to 1880. Their object was to furnish such a detailed account and description of the central and brighter portions of the nebula, that a repetition of it would be easy and short, and so that the question of any future change could be settled definitely. Professor Holden did not consider it of any use to add a new drawing to the great number we already possess, the more so as Bond's engraving (a copy of which accompanies the memoir) must be admitted to give a perfect representation of the nebula, so far as pictorial effect is concerned. The numerous micrometer measures and minute descriptions given by Prof. Holden will unquestionably contribute more to settle the question of variability in the object, than a great number of sketches and drawings could. The variable stars and the relative brightness of the various nebulous masses have been carefully watched, and as changes of brightness were suspected, a series of measures were made with a simple photometer devised by Dr. Hastings, the principal parts of which were a small silvered mirror which threw the light from an illuminated screen (covered with a bit of paper cut from Bond's engraving) upon the image of the nebula. The observations are first given in chronological order, next follows a summary of all the observations from 1656 to 1880 of every separate part of the nebula, with the conclusions which can be drawn from them. Professor Holden comes to the same result as d'Arrest, that there is no proof whatever of any changes of *form* (although in one or two cases such may possibly have occurred), while there is clear evidence of many changes of brightness. We are particularly pleased at seeing the Author arrive at this conclusion, as we have for years been of opinion that all well authenticated changes in nebulæ are changes of brightness and not of form. We would recommend people who are inclined to criticise Bond's and Lord Rosse's drawings, to bear in mind the undoubted changes of relative brightness of many of the masses of the nebula of Orion which have occurred even during the last twenty years.

Among the many points of special interest in the memoir we shall only mention, that the Washington refractor never has shown more than the six stars in the trapezium, nor any new stars very close to it. Mr. Burnham, with the 18-inch Chicago refractor, and the writer of this review with the 27-inch Vienna refractor (in Dublin in 1881), and other observers with powerful instruments have come to the same result. In addition to Bond's engraving and numerous woodcuts the memoir also contains a plate giving an autotype enlargement of the late Henry Draper's photograph of the nebula (see *Copernicus*, Vol. I., p. 160.)

Publications of the Washburn Observatory of the University of Wisconsin, Vol. I. (Madison 1882, 180 pp. 8vo.) The observations were commenced at the Washburn Observatory on April 23, 1881, and the present volume only contains those made up to September 30, 1881, by Professor Holden and Mr. Burnham. The first chapter gives a full description of the buildings and instruments. The Equatoreal, by Clark, has an aperture of 15·56 inches and a focal length of 20 feet; the Meridian Circle, by Repsold, has circles of 22 inches diameter and an object-glass (by Clark) of 4·8 inches aperture. Close to the main building there is a Students' Observatory in which are Mr. Burnham's 6-inch refractor and a transit instrument. In 1881 a number of zones were observed with the 15½-inch Equatoreal and an eyepiece having a field of 25′·5 and a power of 145. As results of these zones we find a list of 23 new Nebulæ and a list of 60 new Double Stars, but in addition to these there is another list of 88 new Double Stars discovered and measured at Madison by Mr. Burnham, who besides took a large number of measures of previously known but difficult Double Stars. Next follow a list of 84 red stars, 27 of which are new (and with the exception of one $21^h\ 0^m\ 33^s$, − 16° 54′, 8·5 mag., already given in *Copernicus*, Vol. I., p. 176) and twelve rough woodcuts of Comet III., 1881. The volume also contains a catalogue of 195 miscellaneous stars from meridian observations made by Mr. Schaeberle at Ann Arbor.

Comète 1882, II.

Fig. 1.

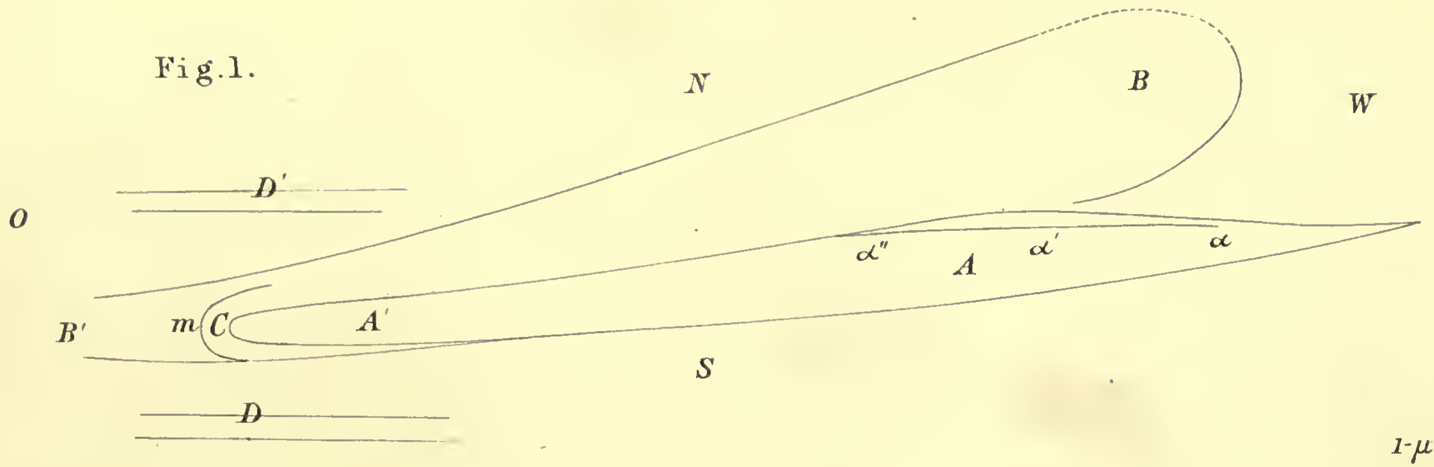


Fig. 2.

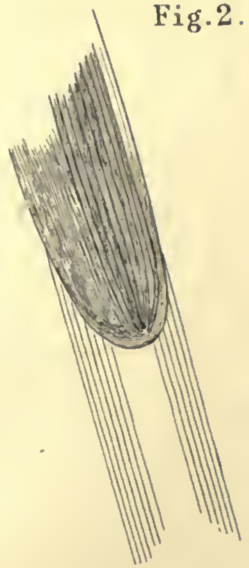
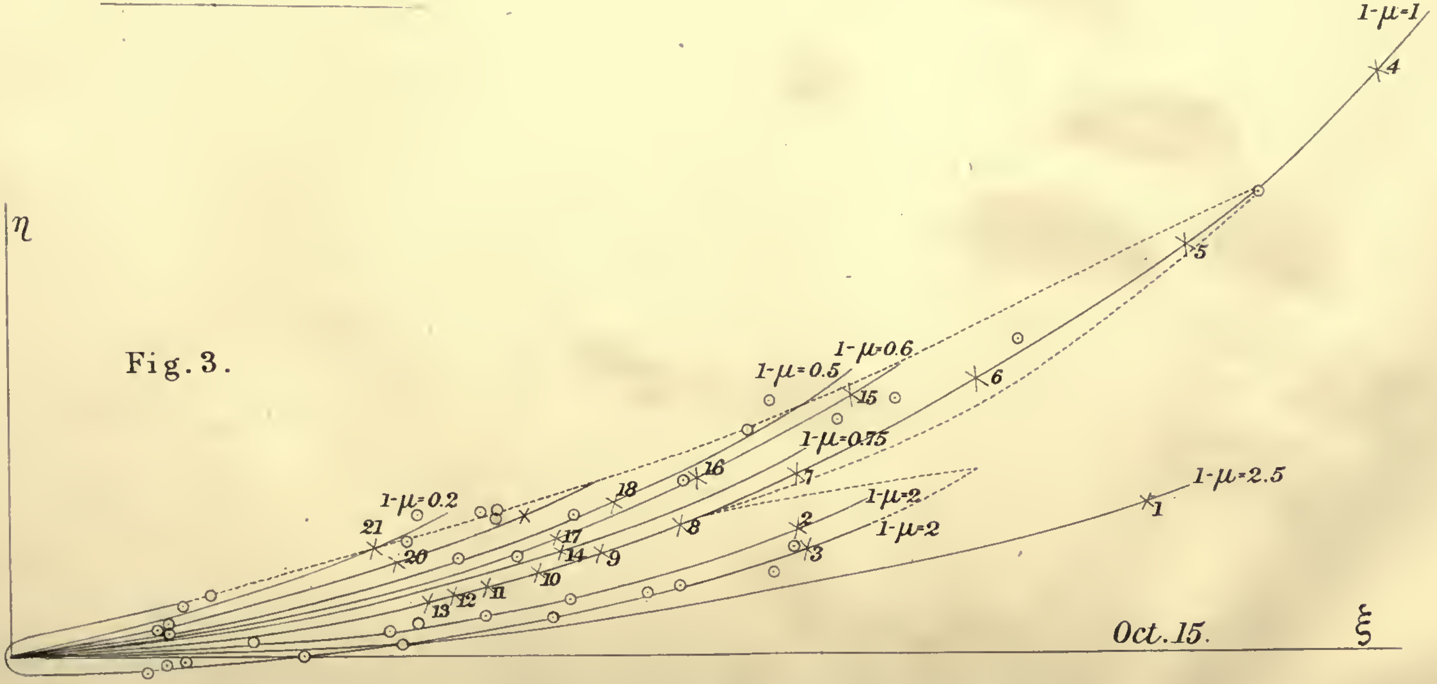


Fig. 3.



Malby & Sons, Lith.

RECHERCHES SUR LA COMÈTE DE 1882, II.

PAR TH. BREDICHIN.

Avec deux planches.

Les traits principaux des phénomènes, présentés par la magnifique comète de septembre 1882 sont habilement réunis par l'éminent observateur M. J. Schmidt dans son contour de la comète publié dans les Astron. Nachrichten (No. 2478) et dans la description succincte de ce contour. Les autres observations, dessins et photographies complètent la description de Schmidt, eclairsissent quelques particularités dans la structure de la queue et donnent, comme par exemple les observations de Moscou—les positions de ses parties par rapport aux étoiles.

C'est par cette raison que je réproduis ici avant tout l'esquisse et la note de Schmidt et puis, pour éviter la fréquente répétition des citations, concernant telle ou telle particularité, je donne simplement une série des extraits de toutes les descriptions qui peuvent faciliter la conception du phénomène dans son développement successif.

§ 1. Schmidt (l c.) ". . . werde ich jetzt in bestimmter Weise angeben, wie die bisher unbekannte Erscheinung von mir in der Zeit von Oct. 4 bis Nov. 21 gesehen und jedesmal gezeichnet ward (Pl. II., fig. 1).

AA′ ist der Hauptcomet, am Sucher bis 23° lang, BB′ die schwächere den Cometen umhüllende Lichtmasse, die ich das Nebelrohr nenne, weil der Anblick, besonders in der Nähe von C, dem Kopfe des Cometen, einen andern Vergleich gar nicht aufkommen liess. Von C bis B′, also östlich über den Cometen hinaus und gegen die Sonne hin sah ich das 1° breite Nebelrohr (bis Nov. 21) jedesmal auch mit freiem Auge, 1° bis 2° weit, wenn das Mondlicht nicht hinderte; 3° bis 5° dagegen am Sucher.

Ausserdem gewahrte ich am Sucher und am Refractor, Oct. 7, 8, 9, 10 und später nicht wieder, den Halo *m ausserhalb* der Region der *gewöhnlichen* Coma; er war mit dem Kerne des Cometen nahe concentrisch.

Die beiderseitig mit den Seiten des Nebelrohres parallel gestellten geraden Nebelarme oder Säume DD′ sah ich zuerst Oct 10. Im Mondlicht schwanden sie viel früher, als das Stück B′. Nach Nov. 6 konnte ich D und D′ nicht mehr mit Sicherheit erkennen. Seit Oct. 11 war ich viel bemüht, für diese 2 Nebelarme den vermutheten abgerundeten Abschluss östlich vom Kerne zu entdecken, was mir indessen mit dem sehr schlechten Sucher in keiner Nacht gelungen ist.

Eine Beobachtung Schiaparelli's Oct. 19 (mir damals brieflich mitgetheilt) wird später wohl zu erklären vermögen, wie diese Erscheinung aufzufassen sein möchte. Mir scheint es, dass sich im Perihele eine grossartige und plötzliche Ausströmung bildete, eine wahre Katastrophe, der zufolge der äussere Comet entstand, und an dem wir nur die Reste bis DD′, vielleicht auch das Nebelrohr, erkennen.

Setzt man für October 15 die Entfernung des Cometen von der Erde = 1 (obgleich zu gering), so war der Durchmesser DD′ = 8·75 Durchmesser der Mondbahn, oder ungefähr 873000 g. Meilen; der Durchmesser des Nebelrohrs bei C = 135000 g. M.; der Durchmesser der Coma des Cometen nahe 100000 g. M.

Die dunkle Linie in der Axe des hellen Schweifes war Oct. 24 bereits undeutlich und später unsichtbar. Das westliche Ende der Schweiffigur zeigte am Sucher im Verlaufe von 5 Wochen sich in seinen allgemeinen Umrissen nicht stark veränderlich. Der wahre Schweif, also AA′, die helle südliche Abtheilung, verlief als sehr lichtschwache schmale Spitze, und war an der N. Seite bei $aa'a''$ auffallend heller, als irgend ein benachbarter Theil.

Aus α′ ward seit Nov. 6. eine deutliche im dortigen feinen Lichtnebel *isolirte* 0°·5 breite Wolke, deren Ort ich oft genau vermerkt habe.

Das Westende des Nebelrohres BB′ war von Oct. 4 bis Nov. 21 jederzeit abgerundet, doch zuletzt sehr *verwaschen* und *schwer* in der Zeichnung darzustellen.

In 25 vollständigen Abbildungen, rectificirt nach den Sternen einer seit Oct. 3 entworfenen Karte der bekannten Katalogsterne, sind die Oerter von α′ und B, der Mitte des abgerundeten Theiles, angegeben. Indem ich diese Positionen aus den (für das Aeq. 1850 geltenden) Zeichnungen entnahmd, wurden sie durch Curven dargestellt und ausgeglichen. Beiderlei Werthe werde ich hier mittheilen, denn sie werden zum ersten Male Veranlassung geben zu Untersuchungen über die Bahnbewegung besonderer Nebelmassen innerhalb des Schweifes oder an den Gränzen desselben. In Folgendem bedeutet; α′ einen hellern Punkt im Saume, später eine Nebelwolke an der N. W. Seite des Hauptschweifes; B die Mitte der westlichen Abrundung des Nebelrohres."

Le tuyau mB′ ne présentait point une continuation directe et immédiate de la partie moins claire de la queue CB : le passage de celle-ci à celui-là s'accompagnait d'un affaiblissement brusque de clarté. M. Tempel a très bien remarqué cette circonstance, et je reproduis ici schématiquement un de ses dessins, du 18 Oct. (Pl. II., fig. 2), qu'il m'a amicalement communiqués. Ces dessins seront probablement publiés dans les Monthly Notices. Le tuyau est un appendice beaucoup moins clair, mis auprès du contour paraboloïdal de la tête. Il a une analogie évidente avec le tuyau plus large DD′. La courbe de l'articulation du tuyau avec la tête se distingue parfaitement sur les dessins. On l'aperçoit aussi clairement sur les photographies que M. Gill a faites au Cap de Bonne-Espérance, et qu'il m'a aimablement envoyées. Ces photographies seront aussi publiées dans les Monthly Notices. D'après ces photographies j'ai mesuré entre autre, pour le 20 Oct. l'angle formé par l'axe du tuyau avec la direction de la partie initiale de l'axe de la queue plus claire, c'est à dire avec le rayon vecteur, et j'ai trouvé que le tuyau est décliné de 5°·5 en avant de ce rayon dans le sens de son mouvement. M. Gill dans sa lettre à moi exprime la pensée, que le bout du tuyau vers le soleil avait le contour parabolic : ". . . the *impression* very strongly conveyed by the photographs and by eye observation with a special opera glass is that the termination is parabolic." C'était donc une *cloche* étroite et longue.

M. Cruls à Rio-Janeiro, donne une description très caractéristique de la comète du 25 Septembre (Comptes Rendus, 6 Novembre 1882) :

"Enfin le 25 (sept.), à 4^h du matin, le ciel à l'horizon se montra limpide et permit d'assister à un spectacle d'une beauté au-dessus de toute expression. A ce moment, une partie seulement de la queue émergeait de l'horizon, et l'aspect en était vraiment imposant, car c'était bien plutôt celui d'une colonne de feu que celui d'un faisceau de lumière. La queue était presque verticale sur la ligne de l'horizon et de forme conique assez prononcée, mesurant à sa partie la plus large environ 1° 30′, et à la base 40′. . . . L'examen télescopique de la queue, à mesure que les parties plus voisines du noyau se laissaient voir, montrait, de *toute évidence* et sans qu'il y eût la moindre illusion d'optique, l'aspect d'un *courant* de lumière extrêmement vive, ou se distinguaient des *filets plus lumineux* que les parties voisines, et l'ensemble donnait fortement l'idée que l'on peut se faire d'un *jet* de métal en fusion.

Lorsque le noyau fut à quelque hauteur au-dessus de l'horizon, il se montra extrêmement brillant, et d'un diamètre d'environ 60″ d'arc. Un courant de lumière enveloppait le noyau, le contournait, et des deux côtés, à l'arrière, les deux filets s'elargissant et se confondant l'un dans l'autre constituaient la naissance de la queue qui se prolongeait ensuite en conservant une intensité de lumière très notable sur une extension d'environ 10° à 12°. Suivant la ligne *axiale* de la queue, on remarquait une *trainée plus sombre,* et immédiatement à l'arrière du noyau, on notait un espace presque totalement privé de lumière, et d'une forme allongée qui allait en s'amincissant à partir du noyau sur une longueur d'environ 30′ d'arc Cette particularité, jointe aux caractères généraux que présentait l'aspect de la comète aux environs du noyau, éveillait involontairement l'idée d'un *remous,* tel qu'il s'en produit en aval d'une pile de pont construite au milieu d'un fort courant, ou, plus exactement, rappelait le vide que laisse derrière lui, en traversant l'air, un projectile animé d'une vitesse suffisante. J'ai encore à signaler ici d'abord, la courbure assez sensible que présentait la queue, et dont la convexité était tournée du coté du Sud ; ensuite une différence très marquée dans la netteté des bords, le bord convexe étant assez vif et bien *tranché,* tandis que le bord concave était vague et *estompé ;* examiné sous un faible grossissement (8 à 10 fois) il était plutôt de nature vaporeuse.

Autour du noyau, on notait également une chevelure qui présentait une largeur de prés de 20′ d'arc, mesurée suivant une ligne passant par le noyau et normalement à l'axe de la queue. Cette chevelure était extrêmement peu lumineuse, quoique nettement visible dans le champ de la lunette du grand équatorial munie d'un oculaire amplifiant 60 fois.

Considérée dans le sens de la longueur et sous le rapport de l'intensité lumineuse, la queue se composait d'un faisceau très lumineux, s'élargissant sensiblement à partir du noyau sur une extension d'environ 12°, et qui se terminait en quelque sorte brusquement. Une partie de la queue se prolongeait ensuite, mais sous un aspect tout autre. Du côté convexe où se notait le bord le plus net, un faisceau lumineux très pâle ayant environ en largeur le 0·4 de l'épaisseur de la queue à son extrémité la plus large, s'étendait sur une longueur de 15° environ, ce qui portait la longueur totale de la queue à près de 30°. En réalité donc, et c'est ce qui constituait une des particularités les plus remarquables et saisissantes de l'aspect de la queue, celle-ci était formée d'un faisceau très lumineux, légèrement recourbé, s'élargissant à partir du noyau sur une longueur de 12° et qui se terminait, pour ainsi dire, brusquement, présentant à cette extrémité l'aspect d'une rupture, et puis ensuite se prolongeait sur une longueur de 15°, ce prolongement ayant une largeur beaucoup moindre et l'intensité lumineuse incomparablement plus faible que l'autre moitié de la queue. . . .

Sur le spectre continu se détachait admirablement un groupe de raies brillantes : celles du *sodium* et du *carbone*. . . . Le spectre de la queue reproduisait l'aspect de celui du noyau, les raies étaient seulement beaucoup plus faibles, quoique bien visibles, *tant* celles du sodium que du carbone."

On voit clairement de cette description—et ce qui est très important pour la théorie—que les enveloppes d'émission vers le soleil se formaient *derrière* le noyau par rapport à son mouvement dans l'espace et puis déjà, en devancant le noyau, elles se recourbaient dans la queue et laissaient *devant* le noyau une bande obscure médiane. Il faut noter ici que dans ce temps le noyau s'éloignait du soleil presque en ligne droite (l'angle de la tangente avec le rayon vecteur étant de 7°·5 seulement) avec la vitesse de 8·4 lieues géogr. par seconde. Il est important aussi que les jets lumineux dans la queue avaient une direction longitudinale par rapport à son axe et que le bord *antérieur* de la queue était *bien tranché.*

Les confirmations de toutes ces circonstances importantes et les preuves de l'existence de la bande obscure au milieu de la queue principale nous sont données par plusieurs autres descriptions, dessins et croquis (*Schmidt*, Astr. Nachr., 2468 ; *Riccò*, Nature, angl., Oct. 19 ; *Schwab*, Astr. Nachr., 2497, etc. . . .) et même par les photographies de Gill (oct. 19, 20, 21), faites à l'aide d'un petit objectif.

Dans la description de Cruls nous rencontrons encore une branche de la queue beaucoup plus longue, moins large et incomparablement plus faible que l'autre partie de la queue. Dans cette branche il sera facile, à l'aide du calcul, de reconnaître la queue du I. type, se projetant en perspective sur la queue plus large du II. et du III. types. Notons, que vers ce temps le rayon visuel faisait un angle très aigu avec le plan de l'orbite. Après le 25 sept. cette partie de la queue devint invisible. Les observations de Cruls confirment aussi l'existence des raies spectrales lumineuses non seulement près du noyau et dans la tête, mais aussi dans la queue de la comète.

Riccò (Astr. Nachr., 2466). "Set. 23. Coda lunga 7°, concava verso Nord, *oscura nel mezzo* in tutta la lunghezza; bordi netti, l'australe più *forte* del altro.—Set. 26. Coda splendida fra le nubi, come barra a bordi netti. . . . Set. 27. . . . bordo australe forte netto, boreale più debole piatto; spazio *oscuro* in tutta la lunghezza. . . . Set. 30. La coda appare come doppia, ramo australe più stretto, più vivo, più netto del boreale. Getto largo vivissimo diretto al sole."

Riccò (ibid., 2462). "Il nucleo della cometa che nei primi giorni di osservazioni (21—22) videsi sotto forma a un di presso sferica indi sensibilmente si *allungò*, divenne *oblungo* con lieve curvatura. . . . Sett. 29. Oscurita nell'interno molto decisa. Sett. 30. . . . *l'oscurita* nell'interno della coda è tanto decisa, che pare che il lato australe formi un ramo staccato, più forte e più stretto dell'altro. Ott. 2. . . . l'oscurita interna è diminuita. Ott. 3. . . . l'oscurita interna è poco decisa; bordo sud sempre forte e netto, bordo boreale più debole evanescente nell'oscurità del cielo."

Riccò (ibid., 2469). "La nebulosita uniforme che prima del 10 ottobre formava il lato boreale della coda, più debole in tutta la lunghezza, ed arrivava fino a pari del nucleo, dal 10 ottobre in poi lo sorpasso sempre più: talche attualmente forma un *debole* inviluppo, leggermente *conico*, largo circa 50′, e che si prolunga più di 2° oltre il nucleo, dalla parte opposta alla coda. Questo inviluppo è situato assai eccentricamente verso nord, ed il suo asse è deviato verso sud rispetto l'asse della coda presso il nucleo. Ott. 24." [D'où il est facile de se persuader qu'il est décliné en avant du rayon vecteur.]

Riccò (Nature, Oct. 19; voir ses dessins caractéristiques de la tête et de la queue avec sa *bande obscure* au milieu, faits le 1 octobre.)

Tempel (Astr. Nachr., 2468). ". . . der grosse Comet seit 3 Tagen einen schwachen, breiten, aber sehr scharf begrenzten Schweif nach der Sonne zu zeigt. Meine Skizze vom 19 Oct. Morg. 4^h zeigt ihn 1° breit beim Kopfe und gegen 3° lang."

Vogel (Astr. Nachr., 2466). "Oct. 1. In der Nähe des Kerns war im Fernrohr eine Theilung des Schweifes angedeutet, mit blossem Auge erschien die Mitte desselben in der Längsrichtung von geringerer Intensität. Die dem Horizonte näher stehende Seite des Schweifes war schärfer begrenzt und heller, als die andere." [C'est le bord antérieur.]

Schwab (Astr. Nachr., 2497). "Die auf der Skizze angegebene Mittellinie erscheint als dunkle Theilung. Sept. 27.—Sept 29. Die dunkle Theilung tritt weniger hervor, dagegen zeigt sich links im Schweife *strahlige* Structur (dans la partie postérieure, faible), die schon am 27 vermuthet wurde. Oct. 2. Der rechte Rand (antérieur) immer schärfer und bedeutend heller als der linke. Oct. 16. Die dunkle Linie kaum *noch* im Operngias kenntlich. . . . Febr. 7, 1883. Schweif für das *blosse* Auge schwierig. . . . Breite 0°·8, Länge = 4°·5."

Eddie (Nature, Nov. 16, 1882). "On the morning of Sept. 24, Mr. Eddie (Cape Colony) says:—When the head had fully risen above the horizon at 4h. 43m. A.M., there were about twenty-five degrees in length of intensely luminous matter, stretching upwards from a still more luminous head. . . . The head appeared as before, to consist of an apparently very solid though not very large nucleus, surrounded by a dense coma of no great extent, especially preceding the nucleus. . . . On the morning of Oct. 3, when, on directing his 9·5 inch Calver upon the nucleus, Mr. Eddie saw not one round planetary disk, as he had last seen it, but two distinct ellipsoidal nuclei in juxtaposition."

Russell (Nature, Nov. 16), from Sydney Morning Herald of Sept. 19 :—"In the large telescope the nucleus appears round and well defined, and measured three seconds in diameter; from it, extended on each side, the first branches of the coma, like two little cherub wings, and in front, the great body of the coma, forming a brillant and symmetrical head, and thence *turning* to form the tail six minutes long. Under close scrunity it was evident that the coma had one or more dark bands, curved like the outline."

Elkin (Month. Not., Nov. 1882). "On Sept. 8 the nucleus itself being a nebulous mass strongly condensed in the centre, some 10″ or 15″ in diameter, and admitting of very accurate observation with heliometer as such."

Tebbutt (ibidem) . . . "on the morning of the 9 and 10 (Sept.) The nucleus was very large and remarkably brillant, and the tail about three or four degrees in length."

Common (Nature, 14 Dec. 1882.) "Sept. 16, 22h. 45m. The direction of the comet was to the centre of the sun, as far as could be estimated." [Le dessin de M. Common réprésente le noyau parfaitement rond, situé dans le creux obscur du paraboloïde de la queue et à son foyer.]

Schur (Astr. Nachr., 2492). "Febr. 5, 1883. Der Comet hat zwei feine fixsternartige Kerne, und zwar lag Febr. 5 der schwächere gegen den helleren im Pos W. 246° und um 38″ entfernter."

Les descriptions ci-dessus nous montrent, que le noyau s'est divisé après le 16 sept. et probablement avant le 23.

Comptes Rendus (25 Sept. 1882). . . . "En Espagne, en Portugal, au midi de la France, en Algérie et en Italie la comète était visible à l'oeil nu en plein midi le dimanche, 17 Sept., à partir de 10h. du matin, le lundi 18, et le mardi 19. . . . Le 18, la position approchée était de 3° à l'ouest du soleil. La chevelure et la partie de la queue visible à l'oeil nu avaient une longueur d'à peu près 20′. Leur contour extérieur affectait la forme d'une demi-ellipse . . . et le noyau, très brillant et assez gros, occupait une position intermédiaire entre le sommet et le foyer."

Gill (Observatory and Month. Not.) "M. Gill was able to inform the Astronomer Royal that, on Sunday, Sept. 17, the comet was followed by two observers (Elkin, Finlay) with separate instruments, right up to the sun's limb, where it suddenly disappeared at 4h. 50m. 58s. Cape Mean Time."

Barnard (Astr. Nachr., 2489). "Oct. 13. The comet is seen to have two short filmy tails pointing towards the sun and opposite to the main train. They are parallel to each other and the space *between* is almost *free* of cometary *haze.* Sept. 23. The south side is the brightest and best defined. The axis of the train *less* bright than other parts. 24 Sept. Inner part darker, the edges bright and clearly defined. Oct. 1. The southern side well defined and bright, the northern side much less so. Oct. 4. A dark channel appears behind the nucleus near the middle of the head, resembling a shadow of the nucleus. Oct. 5 . . . dark channel still seen. Oct 6. Faint channel still seen."

Sur le dessin de Barnard on distingue parfaitement la déviation de la queue anomale en avant du rayon vecteur.

Seabroke (Nature, Oct. 26.) "Oct. 23. . . . The *lower* edge (antérieur) of the tail was very sharp, but the *upper* edge was gradually shaded off."

Watts (ibid. Nov. 2). "Oct. 22. Its *under* boundary was a *well* defined line about 40° from horizontal, and was slightly convex downwards. The *upper* boundary (postérieur) was about 45° from the horizontal, was nearly straight, but very ill-defined, the light fading away into darkness very gradually upwards."

Hopkins (Nat. 23 Nov.) “Nov. 4. The general form of the tail being very similar to the Greek character γ. (Voir le contour de Schmidt.) The southern side still remained brighter than the northern.

Schmidt (Astr. Nachr., 2495). “Jan. 10, 1883. Der 8° lange Schweif zieht über o Canis. . . . Jan. 28. Um 9h. war der 5°·5 lange Schweif gut dem freien Auge sichtbar. Der Zielpunkt der Mittellinie lag 1° südlich von Sirius.”

Copeland (Copernicus, n. 24). “But not merely an odd one, nay, all the six brighter lines observed in the green and yellow correspond to the most prominent Iron lines in that part of the spectrum and even the two broad bands, noticed there, fall into a group of numerous Iron lines.”

Millosewich (Astr. Nachr., 2464). “Dalle osservazioni spettrali di Tacchini il 30 sett. e mie il primo ott. ebbesi : spettro continuo, nucleo vivissimo dalla parte del rosso ; riga del sodio ; bande degli idrocarburi assai ben marcate.”

Thollon (Compt. R., 5 févr. 1883). “A l'aide d'un spectroscope à un seul prisme, nous avons pu constater (le 18 Sept.) d'une manière certaine, non seulement l'existence des raies brillantes du *sodium*, dans le spectre cométaire, mais encore leur déplacement du côté du rouge, déplacement que nous avons évalué à $\frac{1}{4}$ ou $\frac{1}{5}$ de l'intervalle entre les deux raies du sodium. L'examen de ce tableau (temps, distances à la terre et vitesses moyennes) montre que, au moment de nos observations (3h. du soir environ) la comète s'éloignait de la terre, et qu'elle s'éloignait avec une vitesse moyenne de 75 k.m. par seconde, nombre compris entre nos deux évaluations 76 et 61 k. m. . . . les vapeurs de sodium qui entouraient le noyau de la comète suivaient la même trajectoire et faisaient partie des éléments constituants de cet astre.”

Outre les dessins et les descriptions mentionnés plus haut, je dois signaler les dessins et les notes de M. Terby (Bulletin de l'Académie de Belgique).

Le 9, le 10 et le 11 octobre M. Schmidt a vu au sud-ouest de la tête et à quelques dégrés de distance d'elle, une masse nébuleuse (Astr. Nachr., 2468), laquelle, d'après les calculs faits par M. Oppenheim (ibid., 2466), se trouvait dans le plan de l'orbite du noyau et au dedans de cette orbite. La distance de la masse au noyau était égale presque à 0·2, et son rayon vecteur était à peu près égal à celui du noyau. La même masse a été vue par Barnard (ibid., 2489) le 14 oct. Nous allons la mentionner plus tard.

§ 2. Les observations de Moscou sont faites par moi (du 5 oct. au 15 déc.) avec l'assistance des MM. Bélopolsky et Socoloff. Nous nous sommes servis principalement de bonnes jumelles et nous avions porté nos notes sur les copies de l'une des cartes de l'Uranometria Argentina. D'après ces notes je composais le même jour le dessin de la queue, que je transportait moi même plus tard sur la pierre lithographique. Les dessins du noyau sont faits à l'aide de notre réfracteur. Quoique le réseau des coordonnées n'est pas tracé sur mes dessins, il est très facile d'identifier les étoiles qu'on y rencontre, car je n'ai point changé l'échelle de l'Uranometria.

Mes observations se rapportent aux moments suivants du temps moyen. de Moscou, qui plus tard sont réduits au méridien de Greenwich—

Oct. 5	17^{h}	0^{m}	Oct. 12	16^{h}	20^{m}	Nov. 4	16^{h}	30^{m}	Déc. 13	14^{h}
7	16	30	13	16	20	9	16	0	14	14
8	16	30	15	16	20	11	16	50	15	14
9	16	20	16	16	20					

Les dessins du 13, 14 et 15 décembre ne sont pas reproduits : à ces dates la queue avait la forme du 9 novembre, seulement elle était très faible. Le 11 novembre les nuages m'ont empêché de finir le dessin, mais j'ai noté deux étoiles sur les bords de la queue.

Je dois remarquer ici catégoriquement, que les parties claires de la queue, surtout près de son extrêmité, se composaient de filaments, de fibres, qui avaient toujours une direction longitudinale par rapport à l'axe de la queue.

Les points du bord antérieur de la queue sont désignés plus bas par la lettre A ; ceux du bord postérieur par P, et les points du milieu, ou du bord postérieur de la queue plus claire, par la lettre M.

Les observations au mois de janvier 1883 sont celles de M. Schmidt (voir plus haut).

Etoiles observées aux bords de la queue et à son milieu—

	A	P	M
1882 Oct. 5	c	a	—
7	g	b	—
7	x	—	—
8	g	d	c
9	k	h	j
9	i	—	—
12	m	e	l
12	n	—	—
12	$\frac{1}{2}(o+p)$	—	—
13	q	e'	$\frac{1}{2}(l+s)$
13	p	r	—
15	u	s	n
15	v	k	—
15	—	g	—
15	—	t	—
16	u	m	—
16	v	k	—
16	—	g	—
16	—	t	—
Nov. 4	$\frac{1}{3}(\epsilon+\delta+\eta)$	w	α
4	—	β	—
9	ι	θ	
9	μ	λ	—
9	AR. $\nu + 30'$, Dec. $\nu + 30'$	—	
11	π	λ	—
Déc. 13	AR. C $-$ 9′, Dec. C.	AR. A $-$ 22′, Dec. A	—
14	E	AR. B + 14, Dec. B	—
15	$\frac{1}{2}$(E + H)	C	—
1883 Jan. 10	—	—	o Canis majoris.
28	—	—	AR = AR. Sirii, Decl. = Dec. Sir. $- 1^{\circ}$

Positions moyennes des étoiles de comparaison pour 1882·0—

	a	δ		a	δ
a	146° 40′·3	— 7° 33′·1	*o*	138° 28′·5	— 10° 49′.2
b	147 29·0	8 16·7	*p*	138 31·8	11 28·8
c	145 44·3	9 22·4	*v*	135 53·3	11 53·0
e	145 20·3	9 58·9	*w*	138 21·5	16 18·1
é	145 21·3	10 12·1	α	140 21·0	18 38·8
g	141 28·8	10 2·2	θ	141 58·3	20 35·7
h	143 1·5	8 53·8	ι	140 30·3	21 49·6
d	141 49·0	7 59·0	β	131 44·0	12 47·5
i	151 2·8	11 31·1	λ	130 12·3	13 7·2
l	151 13·0	11 46·4	δ	132 46·8	16 15·4
x	150 40·5	10 32·6	ε	132 51·3	16 58·9
s	151 5·8	12 14·1	η	131 55·0	16 30·4
j	152 35·5	10 37·1	μ	133 34·5	18 44·7
m	149 51·0	12 29·7	ν	125 29·5	14 33·0
q	147 13·0	12 23·1	π	130 20·8	18 16·6
k	145 57·5	11 14·4	A	120 38·0	23 58·0
n	142 17·3	11 35·8	B	118 30·8	22 59·6
r	140 39·3	9 28·5	C	117 57·0	22 34·0
t	139 17·0	9 20·2	E	117 9·5	22 56·7
u	141 49·5	12 59·9	H	115 10·7	22 54·9
o Can.	104 31·4	— 23 39·6	Sir.	99 59·0	— 16 33·3

Les éléments paraboliques du noyau (Chandler, Astr. Nachr., 2470) sont—

$$\begin{aligned} T &= \text{Sept. } 17{\cdot}22013 \text{ t. m. Gr.} \\ \pi - \Omega &= 69^\circ\ 28'\ 46''{\cdot}4 \\ \Omega &= 345\ \ 53\ \ 40{\cdot}4 \\ i &= 141\ \ 55\ \ 15{\cdot}0 \\ \lg. q &= 7{\cdot}8915778 \end{aligned} \left.\vphantom{\begin{matrix}a\\a\\a\end{matrix}}\right\} \text{éq. m. } 1882{\cdot}0$$

$$\begin{aligned} x &= r\,(9{\cdot}9950379) \operatorname{Sin}\,(170^\circ\ 40'\ 5''{\cdot}7 + v) \\ y &= r\,(9{\cdot}9876182) \operatorname{Sin}\,(262\ \ 46\ \ 43{\cdot}7 + v) \\ z &= r\,(9{\cdot}4460963) \operatorname{Sin}\,(\ 49\ \ 9\ \ 30{\cdot}9 + v) \end{aligned}$$

La Terre a passé par le plan de l'orbite le 8 Sept. à $4^h\ 48^m$ t. m. Gr.

Les réductions au plan de l'orbite se sont effectuées à l'aide des formules connues (Astr. Nach., 1172).

Avec $\varepsilon = 23^\circ\ 27'\ 17''$ on a $A = 237^\circ\ 26'\ 37''$ et $D = -\ 73^\circ\ 46'\ 51''$

Et puis—

T. m. Green.	*ak*	δ*k*	*a*	δ	Bord	*p*°
Oct. 5·60	159° 51′·4	— 8° 57′·7	155° 44′·3	— 9° 22′·4	A	265° 2′·1
			146 40·3	7 33·1	P	
” 7·58	159 9·3	9 59·0	141 28·8	10 2·2	A	265 44·9
”			150 40·5	10 32·6	A	
			147 29·0	8 16·7	P	
” 8·58	158 49·7	10 20·8	141 28·8	10 2·2	A	266 6·7
			141 49·0	2 59·0	P	
”			145 44·3	9 22·4	M	
” 9·58	158 30·9	10 47·0	145 57·5	11 14·4	A	266 28·9
”			151 2·8	11 31·1	A	
”			143 1·5	8 53·8	P	
”			152 35·5	10 37·1	A	

T. m. Green.	αk	δk	α	δ	Bord.	p°
Oct. 12·58	157° 35′·7	— 12° 4′·4	149° 51′·0	— 12° 29′·7	A	267° 36′·9
„			142 17·3	11 35·8	A	
„			138 30·2	11 9·0	A	
„			145 20·3	9 58·9	P	
„			151 13·0	11 46·4	M	
13·58	157 17·8	12 29·7	147 13·0	12 23·1	A	268 0·4
„			138 31·8	11 28·8	A	
„			145 21·3	10 12·1	P	
„			140 39·3	9 28·5	P	
„			151 9·4	12 0·3	M	
15·58	156 42·2	13 19·9	141 49·5	12 59·9	A	268 47·9
„			135 53·3	11 53·0	A	
„			151 5·8	12 14·1	P	
„			145 57·5	11 14·4	P	
„			141 28·8	10 2·2	P	
„			139 17·0	9 20·2	P	
„			142 17·3	11 35·8	M	
16·58	156 24·4	— 13 44·8	141 49·5	12 59·9	A	269 12·0
„			135 53·3	11 53·0	A	
„			149 51·0	12 29·7	P	
„			145 57·5	11 14·4	P	
„			141 28·8	10 2·2	P	
„			139 17·0	9 20·2	P	
Nov. 4·58	147 27·9	21 0·5	132 31·0	16 34·9	A	279 10·9
„			138 21·5	16 18·1	P	
			131 44·0	12 47·5	P	
„			140 21·0	18 38·8	M	
9·56	145 13·1	22 43·6	140 30·3	21 49·6	A	282 21·6
„			133 34·5	18 44·7	A	
„			125 59·5	14 3·0	A	
„			141 58·3	20 35·7	P	
„			130 12·3	13 7·2	P	
11·60	144 13·4	23 24·4	130 20·8	18 16·6	A	283 46·5
„			130 12·3	13 7·2	P	
Déc. 13·48	122 49·1	30 20·7	117 48·0	22 34·0	A	318 23·2
„			120 16·0	23 58·0	P	
14·48	122 1·0	30 23·5	117 9·5	22 56·7	A	319 58·3
„			118 44·8	22 59·6	P	
15·48	121 12·8	30 26·3	116 10·2	22 55·8	A	321 33·4
„			117 57·0	22 34·0	P	
Jan. 10·4	102 10·3	27 40·1	104 31·4	23 39·6	M	12 10·8
28·4	93 39·3	— 23 4·6	99 59·0	— 17 33·3	M	45 5·5

On a plus loin—

	lg ρ	P	P′	S
Oct. 5·60	0·123131	163° 47′·9	99° 23′·7	102° 5′·3
7·58	0·127796	163 43·0	98 29·4	102 45·9
8·58	0·129993	163 40·4	98 2·6	103 6·2
9·58	0·131941	163 37·9	97 37·0	103 25·9
12·58	0·137245	163 30·3	96 20·8	104 26·3
13·58	0·138724	163 28·1	95 56·1	104 45·0
15·58	0·141463	163 23·9	95 6·6	105 22·6
16·58	0·142728	163 21·8	94 42·1	105 41·5
Nov. 4·58	0·170825	162 39·8	83 53·8	110 8·4
9·56	0·172574	162 35·1	81 13·4	111 9·5
11·60	0·173158	162 33·0	80 4·0	111 32·2
Déc. 13·48	0·195586	164 1·9	58 12·0	112 37·2
14·48	0·197145	164 9·5	57 28·0	112 28·6
15·48	0·198704	164 17·1	56 44·0	112 20·0
Janv. 10·4	0·26631	168 13·3	—	105 40·1
28·4	0·33701	170 21·7	—	99 44·1

Les angles ϕ pour les observations d'octobre sont réduits à l'époque du 15·58 octobre; les ϕ de novembre sont réduits au 9·56 novembre. Pour décembre ces réductions sont insignifiantes et les coordonnées du 13, 14 et 15 déc. sont réunies ensemble pour le 14. Les formules pour les réductions $\delta\phi$ se trouvent dans ce journal (Vol. II., p. 7). Pour la construction graphique j'ai pris l'échelle où 1 = 75 millim.

	p	s	T	φ	lg Δ	δφ	φ′	Δ mil.
Oct. 5	267° 11′·2	13° 56′·8	137° 13′·9	+ 5° 10′·0	9·82206	− 82′·1	A + 3° 47′·9	49·8
	275 10·5	13 9·7	149 33·8	+ 18 21·7	0·00779	284·9	P + 13 36·8	76·4
7	268 16·6	17 24·1	137 58·5	+ 5 38·7	9·98380	72·8	A + 4 25·9	72·3
	265 24·2	8 21·8	131 48·0	− 0 51·4	9·48393	52·2	A − 1 43·6	22·9
	277 27·2	11 38·8	150 37·7	+ 19 21·2	9·94938	193·6	P + 16 7·6	66·8
8	269 29·1	17 5·4	139 29·5	+ 7 6·4	9·99888	61·9	A + 6 4·5	74·8
	276 34·3	16 57·3	149 6·6	+ 17 33·8	0·21312	129·2	P + 15 24·6	122·5
	273 10·4	12 56·0	145 6·6	+ 13 9·6	9·90712	75·1	M + 11 54·5	60·5
9	266 41·4	12 18·4	133 24·8	+ 0 28·7	9·70992	37·4	A − 0 8·7	38·5
	263 33·8	7 21·8	125 50·0	− 7 28·3	9·37696	25·0	A − 7 53·3	17·9
	275 49·7	15 22·8	147 42·0	+ 15 52·0	0·09161	95·8	P + 14 16·5	92·6
	271 30·2	5 49·5	142 6·7	+ 9 45·1	9·41337	36·1	M + 9 9·0	19·5
12	265 59·3	7 34·7	130 0·8	− 3 37·8	9·42852	12·0	A − 3 49·8	20·1
	270 13·1	15 46·3	138 9·6	+ 5 3·7	9·92860	21·1	A + 4 42·6	63·7
	270 51·6	18 44·1	139 12·0	+ 6 11·1	0·06923	24·7	A + 5 46·4	87·9
	278 38·8	12 12·6	148 47·0	+ 16 47·2	9·94976	61·9	P + 15 45·3	66·8
	272 5·2	6 14·8	141 3·7	+ 8 12·3	9·44143	16·8	M + 7 55·5	20·8
13	269 32·9	9 50·3	136 27·1	+ 3 1·2	9·62710	9·8	A + 2 51·4	31·8
	271 10·1	18 23·3	139 6·5	+ 5 53·5	0·05475	16·0	A + 5 37·5	85·1
	279 51·5	11 55·8	149 21·6	17 18·6	9·94789	40·3	P 16 38·3	66·5
	278 48·1	16 36·6	148 23·7	16 12·1	0·12801	30·5	P 15 41·6	114·1
	274 1·0	6 1·3	143 7·6	10 17·3	9·44955	− 10·8	M 10 6·5	21·1
15	269 36·4	14 29·6	135 25·6	1 32·3	9·83984	—	A 1 32·3	51·9
	271 44·3	20 21·7	138 50·8	5 15·2	0·13278	—	A 5 15·2	101·8
	280 42·9	5 34·6	149 4·0	16 44·1	9·49733	—	P 16 44·1	23·6
	280 5·4	10 42·2	148 31·4	16 6·1	9·86049	—	P 16 6·1	54·4
	280 50·5	15 15·8	149 10·5	16 51·6	0·13326	—	P 16 51·6	101·9
	281 20·3	17 32·2	149 35·4	17 20·8	0·27257	—	P 17 20·8	140·5
	275 31·8	14 10·9	143 52·3	10 48·3	9·95804	—	M 10 48·3	68·1
16	271 18·6	14 12·5	137 38·7	4 44·2	9·85900	+ 5·9	A 4 50·1	54·2
	272 57·8	20 5·5	140 3·3	7 22·9	0·14769	+ 8·2	A 7 31·1	105·4
	280 19·8	6 30·4	148 13·4	16 36·4	9·56672	+ 12·2	P 16 48·7	27·7
	282 37·9	10 30·2	150 7·0	18 50·4	9·88258	+ 17·0	P 19 7·4	57·3
	282 37·4	15 4·0	150 6·7	17 52·0	0·14970	+ 13·6	P 18 5·6	105·9
	285 29·0	17 35·2	152 9·0	20 19·7	0·37211	+ 16·9	P 20 36·6	176·7
Nov. 4	284 51·2	14 49·2	145 27·3	5 55·8	0·05042	− 13·2	A 5 42·6	84·3
	297 5·7	9 49·5	152 21·0	15 15·4	9·91704	− 33·1	P 14 42·3	61·9
	296 9·1	17 7·9	151 57·0	14 39·5	0·36261	− 32·2	P 14 7·3	172·9
	288 11·9	7 5·8	147 45·0	+ 8 52·2	9·63422	− 11·4	M + 8 40·8	32·3
9	280 45·8	4 27·2	140 39·7	− 1 43·5	9·30535	—	A − 1 43·5	15·2
	287 57·1	9 15·1	146 13·7	+ 5 17·5	9·76072	—	A + 5 17·5	43·3
	292 20·4	20 12·8	148 49·0	8 47·4	0·43161	—	A 8 47·4	202·6
	304 38·0	3 41·7	153 51·4	16 31·4	9·39987	—	P 16 31·4	18·8
	301 24·5	17 11·6	152 47·0	14 43·4	0·40260	—	P 14 43·4	189·6
11	288 59·8	13 56·2	146 23·2	4 50·6	0·02764	+ 5·3	A 4 55·9	80·0
	305 19·2	16 48·3	153 36·6	15 45·9	0·41279	+ 12·9	P 15 58·8	194·0
Dec. 13	328 50	8 59	156 39	4 30	9·99445	—	A 4 30	74·1
	339 50	6 47	157 20	8 51	9·83066	—	P 8 51	50·8
14	328 40	8 38	156 45	3 42	9·97156	—	A 3 42	70·3
	337 42	7 57	157 23	7 16	9·93454	—	P 7 16	64·5
15	327 49	8 44	156 49	2 39	9·98294	—	A 2 39	72·1
	338 53	8 23	157 35	6 59	9·97775	—	P + 6 59	71·3
Jn. 10·4	28 23·5	8 0·0	159 50·6	6 0·0	0·08644	—	M + 6 0·0	—
28·4	48 12·1	5 30·0	162 8·2	+ 1 36·3	9·98794	—	M + 1 36·3	—

N.B. Dans les calculs de $\delta\phi$, faits après l'évaluation des forces, j'ai employé—

pour le bord antérieur	A	$1 - \mu = 2{\cdot}0$	et lg F = 1·02863
pour le milieu	M	1·0	1·17915
pour le bord post. P et	$\Delta < 1$	0·2	1·52863
pour le même bord et	$\Delta > 1$	0·6	1·29007

Les points observés sont portés sur les planches II. et III. (figg. 3, 4, 5), où ils sont désignés par des petits cercles.

Pour les nuages de Schmidt B et α' on a pour l'équinoxe de 1882·0—

B

	αk	δk	α	δ	$p°$
Oct. 9·68	158° 29′·0	− 10° 49′·6	143° 35′·8	− 9° 5′·9	266° 31′·3
15·68	156 40·2	13 22·6	140 19·0	10 22·3	268 50·5
20·68	155 9·7	15 25·4	138 19·0	11 9·8	270 55·0
Nov. 9·68	145 9·6	− 22 45·9	131 32·3	− 12 34·1	282 26·6

α'

Oct. 9·68	158 29·0	− 10 49·6	144 56·5	− 10 5·2	266 31·3
15·68	156 40·2	13 22·6	141 4·3	11 16·5	268 50·5
20·68	155 9·7	15 25·4	138 48·1	12 33·3	270 55·0
Nov. 9·68	145 9·6	− 22 45·9	130 30·4	− 14 43·4	282 26·6

Et puis—

	lg. ρ	P	P′	S
Oct. 9	0·132135	163° 37′·7	97° 34′·2	103° 28′·1
15	0·141601	163 23·6	95 4·0	105 24·6
20	0·146606	163 10·7	92 59·8	106 55·6
Nov. 9	0·172609	162 35·0	81 9·3	111 10·8

A l'aide des r et v pour le noyau il est facile de calculer pour les moments des observations la distance de chaque nuage au soleil R et l'angle de cette distance avec l'axe de l'orbite du noyau V.

	B		α'	
	lg. R	180° − V	lg. R	180° − V
Oct. 9·68	0·28125	20° 1′	0·19773	16° 13′
15·68	0·36601	19 1	0·30425	16 25
20·68	0·41997	18 48	0·34557	15 17
Nov. 9·68	0·61522	18 54	0·53144	14 52

D'après ces valeurs on pourra calculer sans aucune difficulté le chemin S parcouru par chaque nuage dans l'intervalle du 9·68 octobre au 9·68 novembre. Nous y reviendrons plus tard.

§ 3. Calculons maintenant les positions des axes des queues correspondant aux différentes valeurs de la force répulsive, en employant les désignations et les formules connues.

(1) L'époque de l'observation est le 15 octobre.

M	$1-\mu$	M_1	G	g	V_1			t	No.
Oct. 15·58	2·5	Sept. 19·300	0°	0	14°	39′	34′	27·99548	1
"	2·0	19·000	0	0	21	41	46	28·48769	2
"	2·0	19·000	− 90	0·1	23	34	53	28·45920	3
"	1·0	17·475	0	0	—			—	4
"	1·0	17·580	0	0	—			—	5
"	1·0	17·750	0	0	—			—	6
"	1·0	18·000	0	0	—			—	7
"	1·0	18·250	0	0	—			—	8
"	1·0	18·500	0	0	—			—	9
"	1·0	18·750	0	0	—			—	10
"	1·0	19·000	0	0	—			—	11
"	1·0	19·250	0	0	—			—	12
15·58	1·0	19·300	0	0	—			—	13
15·68	0·75	18·228	0	0	100	39	2	28·71737	14
15·58	0·60	17·580	0	0	96	25	54	28·41128	15
15·68	0·60	17·750	0	0	103	55	13	28·55220	16
15·68	0·60	18·000	0	0	110	53	51	28·61059	17
15·68	0·50	17·750	0	0	111	53	51	28·53768	18
15·58	0·30	17·580	0	0	117	36	58	28·38812	19
15·58	0·20	17·580	0	0	123	25	21	28·37866	20
15·58	0·20	17·580	+ 90	0·1	125	49	17	28·37673	21

On voit, qu'au lieu de M = 15·58 Oct. et M = 9·56 Nov. j'ai introduit quelquefois 15·68 et. 9·68 respectivement ; mais cette différence de 0·1 jour reste sans influence dans la construction graphique.

	F			V			lg R	lg Δ	ϕ		Δ mill.
No. 1	88°	38′	3″	23°	37′	20″	0·4741936	0·307833	7°	43′·3	152·4
2	88	52	16	31	50	52	0·3724506	0·149400	9	2·2	105·8
3	88	50	23	34	57	31	0·3737907	0·150748	7	2·1	106·2
4		—			—		0·5487012	0·421393	22	50·0	197·9
5		—			—		0·4942046	0·343152	19	15·8	165·3
6		—			—		0·4335602	0·251212	15	50·8	133·8
7		—			—		0·3745144	0·155070	12	56·1	107·2
8		—			—		0·3331049	0·082469	11	5·3	90·7
9		—			—		0·3014194	0·022712	9	46·5	79·1
10		—			—		0·2759030	9·972626	8	44·7	70·4
11		—			—		0·2546356	9·928132	7	55·3	63·6
12		—			—		0·2364835	9·888191	7	14·0	59·0
13		—			—		0·2301625	9·873943	7	11·3	56·1
14		—			—		0·2875409	9·995855	10	35·6	74·3
15		—			—		0·3952590	0·192239	17	12·5	116·8
16		—			—		0·3406160	0·097182	14	10·3	93·8
17	88	42	58	129	39	41	0·2873301	9·996153	14	34·4	74·4
18	88	44	21	134	44	46	0·3092303	0·039744	13	35·2	82·2
19	88	0	30	146	58	57	0·2749674	9·975248	14	32·2	70·9
20	86	27	26	154	30	5	0·2149015	9·842952	13	5·4	52·3
21	86	12	25	155	58	33	0·2057241	9·823760	15	41·4	50·0

Pour le bord antérieur j'ai pris G = − 90°, et pour le bord postérieur G = + 90°, comme valeurs limites de ces angles. Quant à la vitesse initiale, je l'admet égale à 0·1, car lors des époques adoptées M, le I type n'était pas encore éteint et sa vitesse g = 0·15 augmentait la vitesse des autres types.

(2.) L'époque de l'observation est le 9 novembre. Il est facile de s'aperçevoir, que **1** $-$ μ = 2·5 ne satisfait point les observations d'octobre, donc elle peut être rejetée au novembre.

M	1 − μ	M₁	G	g	V₁	t	No.
Nov. 9·56	2·0	19·000	0°	0	21° 41′ 46″	53·12477	1
9·56	2·0	19·000	− 90	0·1	23 34 53	53·45920	2
9·56	1·0	18·500	0	0	—	—	3
9·68	0·75	18·228	0	0	100 39 2	53·71737	4
9·68	0·6	17·750	0	0	103 55 13	53·55219	5
9·68	0·6	18·000	0	0	110 53 51	53·61059	6
9·68	0·5	17·750	0	0	111 53 51	53·53768	7
9·56	0·3	17·580	0	0	117 36 58	53·36812	8
9·56	0·2	17·580	+ 90	0·1	125 49 17	53·71673	9

	F	V	lg R	lg Δ	φ	Δ mill.
No. 1	89° 24′ 59″	32° 8′ 11″	0·6553438	0·475950	11° 14′·4	224·4
2	89 24 0	35 16 51	0·6567467	0·477186	9 12·1	225·0
3	—	—	0·6473411	0·465923	14 50·2	219·3
4	89 24 41	117 12 47	0·5547680	0·313129	12 35·4	154·3
5	89 29 21	126 20 13	0·6074630	0·404026	15 58·3	190·2
6	89 17 47	130 6 25	0·5524908	0·309815	13 30·3	153·0
7	89 18 30	135 8 57	0·5731492	0·346993	15 24·7	166·8
8	88 53 17	147 27 32	0·5339260	0·279644	16 18·3	142·8
9	87 49 7	156 37 28	0·4575860	0·130618	17 15·5	101·3

(3.) Les observations de decembre ne sont pas assez exactes et pour elles on peut se contenter du calcul avec la seule valeur de 1 $-$ μ = 1.

M	M₁	lg R	lg Δ	φ	Δ mill.	No.
Déc. 14·48	Nov. 1·50	0·3568746	9·014286	1° 4′·2	7·8	1
14·48	Oct. 1·16	0·4569890	9·841452	3 47·8	52·0	2
14·48	Sept. 24·77	0·5352343	0·101003	5 18·3	94·7	3

Les points calculés ci-dessus sont portés sur les planches (figg. 3, 4, 5), où on les trouve facilement à l'aide de leurs numeros et des descriptions.

(4.) Calculs pour l'observation de Cruls. Epoque de l'observation est M = Sept. 25·829—

	1 − μ	M₁	G	g	V₁	t
No. 1	12·0	Sept. 17·475	0	0	4° 2′ 50″	8·37247
2	12·0	17·750	0	0	3 18 16	8·18213
3	12·0	18·000	0	0	2 56 14	7·98854
4	12·0	18·000	+ 90	0·15	2 38 34	7·98908
5	2·0	17·475	0	0	29 47 46	8·53073
6	2·0	17·750	0	0	27 38 27	8·56847
7	1·0	17·475	0	0	—	—

	F	V	lg R	Δ	φ	Δ mill.
No. 1	89° 42′ 17″	15° 50′ 1″	0·548247	3·13571	27° 5′·8	235·2
2	89 18 47	12 8 31	0·411643	2·14848	20 2·2	161·2
3	88 54 35	10 31 35	0·332330	1·70833	15 38·6	128·2
4	88 54 39	9 27 7	0·332057	1·70871	16 37·5	128·2
5	89 23 32	51 31 59	0·165869	1·03920	24 25·0	78·0
6	88 47 33	43 32 52	0·051189	0·68102	15 20·2	51·1
7	—	—	0·030706	0·63805	21 51·6	47·9

Les résultats sont portés sur la planche III. (fig. 6.)

On voit que les particules du I type ($1 - \mu = 12$), sorties du noyau au même instant avec les particules du II type, le 17·475 sept., se trouvent, à l'époque de l'observation, le 25·8 sept. beaucoup plus éloignées du noyau. Cela provient de ce que leurs orbites sont plus *brachychrones* que celles des particules du II type.

En perspective, dont l'action était très forte le 25 sept., comme on le voit d'après les valeurs des angles P et S, la queue plus longue du I type se projetait sur la partie antérieure de la queue du II type.

Les valeurs des anomalies, des rayons vecteurs du noyau et des angles rayon-tangente, employées dans nos calculs sont (je donne ici la valeur approximative de la vitesse orbitale H ; dans les calculs j'ai introduit la valeur exacte de H) :—

		v			lg r	β			H
Sept.	17·475	126°	9′	58″	8·5799580	26°	55′	1″	7·25
	17·580	133	1	55	8·6907336	23	29	3	6·38
	17·662	136	38	51	8·7566744	21	40	35	—
	17·750	139	32	19	8·8139198	20	13	51	5·54
	18·000	144	59	23	8·9350538	17	30	18	4·82
	18·228	148	8	2	9·0144472	15	55	59	—
	18·250	148	22	56	9·0210694	15	48	32	4·37
	18·500	150	46	20	9·0877298	14	36	50	4·04
	18·750	152	34	54	9·1421046	13	42	33	3·80
	19·000	154	1	7	9·1880222	12	59	26	3·60
	19·250	155	11	56	9·2277324	12	24	2	—
	19·300	155	24	40	9·2350810	12	17	40	3·44
	24·774	164	16	1	9·618984				
	25·829	164	57	12	9·6574998				
Oct.	1·16	167	13	33	9·799036				
	9·68	169	7	40	9·9385500				
	15·58	169	56	59	0·0066648				
	15·68	169	57	42	0·0076710				
	20·68	170	29	37	0·0548536				
Nov.	1·50	171	24	30	0·142602				
	9·56	171	52	6	0·190288				
	9·68	171	52	27	0·190928				
Déc.	14·48	173	7	53	0·336682				

§ 4. L'examen de la construction graphique de la queue nous montre, que le conoïde sud de la queue, plus clair et plus dense pour ainsi dire, ayant son creux intérieur, ou sa bande obscure médiane, consistait en particules mues, outre l'attraction, par la force répulsive $1 - \mu = 2·0$ (molécules de quelque hydrocarbure de composition inconnue.)

La partie nord, ce conoïde moins clair, ou plutôt ce faisceau de plusieurs conoïdes inégalement récourbés, contenait des molécules aux forces répulsives de $1 - \mu = 0·75$ (ou de 1.0) à $1 - \mu = 0·2$ (oxygène ? nitrogène ? sodium, fer). Le creux intérieur de ce faisceau a dû être estompé, car les parois d'un conoïde se plaçaient dans l'intérieur d'un autre, et ainsi de suite. De là cette structure radiale, rayée (voir : Cruls, Schwab—"strahlig") ; et même près de la tête les molécules de ces conoïdes subissaient l'action perturbatrice des parois postérieures du conoïde plus dense. Tout cela n'est pas en desaccord, je le suppose, avec les considérations que j'ai déjà énoncées par rapport aux plusieurs comètes et que j'avais graphiquement construites maintc fois (voir, par exemple, *Copernicus*, No. 6, Planches).

Les particules du conoïde clair, sorties du noyau avant le 19 sept. et vues encore par Eddie le 24 sept., ont reussi déjà vers le 5 octobre à devenir imperceptibles, en s'étendant sur leurs hyperboles très brachychrones (je ne dis point brachystochrones.)

Au bout de la partie moins claire on continuait encore à voir les particules sorties vers le 18 sept., car à cette époque ont eu lieu, comme nous le verrons bientôt, deux émissions très abondantes, qui ont formé des condensations nébuleuses claires.

Pour trouver la vitesse initiale g des effluves dans le conoïde clair, calculons ce g pour le 20 oct. ($\lg r = 0{\cdot}02809$, $\lg \rho = 0{\cdot}14611$ et $1 - \mu = 2$), pour quelques valeurs e des rayons au sommet des enveloppes du côté du soleil. L'on a—

pour $e =$	$g =$
$60''$	$0{\cdot}038$
120	$0{\cdot}054$
180	$0{\cdot}066$

Le contour de la tête fait par M. Schmidt (Astr. Nachr., 2499) nous donne, le 20 oct., pour le rayon de l'enveloppe extérieure $= e = 4^{s}{\cdot}83 = 72''{\cdot}5$, d'où $g = 0{\cdot}041$.

M. Peters à Clinton, dans son aimable lettre à moi donne aussi le contour de la tête, pour la même époque, d'où l'on a $e = 80''$ et $g = 0{\cdot}043$; donc en moyenne $g = 0{\cdot}04$, ce qui est d'accord avec les résultats obtenus par moi pour plusieurs autres comètes.

Les particules du III type, soumises à la force $1 - \mu = 0{\cdot}2$, éprouvaient l'impulsion des particules plus nombreuses à vitesse $0{\cdot}04$, et si l'action effective de ces dernières ne communiquait aux premières que la vitesse $g = 0{\cdot}02$, et dans ce cas les particules du III type ont dû former une enveloppe paraboloïdale avec le rayon au sommet égal à $170''$. Dans la description citée de Schmidt on lit, qu'au delà du contour de la chevelure ordinaire (coma) il se voyait un *halos*. Dans cette action on doit aussi voir la cause d'un certain renflement de la racine de la queue moins claire.

J'ai signalé une pareille particularité et je l'ai expliquée de la même manière dans plusieurs comètes, par exemple dans celle de 1881, IV.

Le calcul des observations de Schmidt du 10 et du 28 janvier 1883, exposé plus haut, nous montre, que vers ce temps la longueur de la queue était encore égale à 1, ce qui donne pour la distance de son extrêmité au soleil $3{\cdot}8$; et la moyenne des demi grands axes de Mars et de Jupiter est $3{\cdot}35$.

Le 7 février, quand la queue restait encore visible à l'oeil nu (Schwab) et avait presque la même longueur, la vitesse du noyau n'était égale qu'à $3{\cdot}25$ lieues géogr. par seconde.

§ 5. Revenons maintenant aux nuages dans la queue observés par Schmidt. L'un d'eux formait l'extrémité nord-ouest de la partie plus faible de la queue; l'autre était situé à l'endroit de divergence des deux moitiés de la queue. Du 6 novembre, grâce en partie au changement de perspective, il est devenu plus isolé M. Schmidt a reussi à déterminer les positions de ces nuages, qu'il désigne par B et α' (Astr. Nachr. No. 2478. pp. 89–92), durant un assez long intervalle de temps, et il a réuni ces observations en quatre positions normales pour chaque nuage. Les positions, réduites au 1882·0, sont données plus haut (§ 2).

Il est clair, que ces positions ne peuvent prétendre qu'à une exactitude tout-à-fait relative, tant plus que l'un des objets devint à la fin très estompé et l'autre a subi un changement de forme (voir la description). Ajoutons y encore l'influence variable de la Lune.

A l'aide de plusieurs épreuves je suis parvenu à trouver les valeurs des force répulsives et les orbites hypérboliques pour chacun des nuages.

Pour le nuage B la force répulsive du Soleil est $1 - \mu = 0{\cdot}6$, ou sa force effective $\mu = 0{\cdot}4$, et l'époque de separation des particules du noyau est M_1 = Sept. 17·662; les éléments de l'orbite sont—

$$
\begin{aligned}
T &= \text{Sept. } 17{\cdot}14878 \text{ T. m. de Greenw.}\\
&\left.\begin{aligned}
\pi - \Omega &= 105^\circ\ 38'\ 3''{\cdot}2\\
\Omega &= 345\ \ 53\ \ 40{\cdot}4\\
i &= 141\ \ 55\ \ 15{\cdot}0
\end{aligned}\right\}\text{éq. m. } 1882{\cdot}0\\
\log Q &= 8{\cdot}1519422\\
\log A &= 8{\cdot}2795531\\
\log E &= 0{\cdot}2418949
\end{aligned}
$$

où T est le temps du passage au périhélie hyperbolique. Les éléments paraboliques du noyau sont ceux de M. Chandler (Astr. Nachr. No. 2470).

Les coordonnées pour le calcul des ascensions droites et des déclinaisons α et δ du nuage seront—

$$
\begin{aligned}
x &= R.\ (9{\cdot}9950379)\ \text{Sin}\ (206^\circ\ 49'\ 22''{\cdot}5 + V)\\
y &= R.\ (9{\cdot}9876182)\ \text{Sin}\ (298\ \ 56\ \ \ 0{\cdot}5 + V)\\
z &= R.\ (9{\cdot}4460963)\ \text{Sin}\ (\ 85\ \ 18\ \ 47{\cdot}7 + V)
\end{aligned}
$$

Les nombres en parenthèses sont des logarithmes. Il est presque inutile d'ajouter, que R et V sont les rayons vecteurs et les anomalies vraies hyperboliques, qu'on calcule d'après les formules que j'ai données dans ce journal, Vol. I., pp. 107–108.

Pour le nuage α^1: $1 - \mu = 0{\cdot}75$, M_1 = Sept. 18·228.

$$
\begin{aligned}
T &= \text{Sept. } 16{\cdot}96263\\
\pi - \Omega &= 116^\circ\ 57'\ 46''{\cdot}1\\
\Omega &= 345\ \ 53\ \ 40{\cdot}4\\
i &= 141\ \ 55\ \ 15{\cdot}0\\
\log Q &= 8{\cdot}2965274\\
\log A &= 8{\cdot}2362959\\
\log E &= 0{\cdot}3321891
\end{aligned}
$$

$$
\begin{aligned}
x &= R.\ (9{\cdot}9950379)\ \text{Sin}\ (218^\circ\ \ 9'\ \ 5''{\cdot}4 + V)\\
y &= R.\ (9{\cdot}9876182)\ \text{Sin}\ (310\ \ 15\ \ 43{\cdot}4 + V)\\
z &= R.\ (9{\cdot}4460963)\ \text{Sin}\ (\ 96\ \ 38\ \ 30{\cdot}6 + V)
\end{aligned}
$$

Si l'on calcule α et δ pour les moments d'observation donnés plus haut et si on les compare avec les coordonnées α et δ observés, on aura les coordonnées suivantes calculées et les différences entre le calcul et l'observation ($c - o$)—

	B				a^1			
	α	$c - o$	δ	$c - o$	α	$c - o$	δ	$c - o$
Oct. 9	144° 3′·4	+ 27′·6	− 9° 11′·4	− 5′·5	145° 14′·2	+ 17′·7	− 10° 10′·4	− 5′·2
„ 15	140 39·2	+ 20·2	10 14·3	+ 8·0	141 20·9	+ 16·6	11 31·8	− 15·3
„ 20	138 17·5	− 1·5	10 53·3	+ 16·5	138 34·8	− 13·3	12 23·7	+ 9·6
Nov. 9	131 18·5	− 13·8	− 12 17·0	+ 16·9	130 2·0	− 28·4	− 14 19·0	+ 24·4

La somme des carrés des erreurs pour B est 2128 et pour a^1 2585.

Chaque changement dans l'époque M_1 et dans la force $1 - \mu$ rend plus grande la somme des carrés des erreurs. Les erreurs modiques restantes peuvent être expliquées en grande partie et par une raréfaction ou dissolution progressive des nuages à leurs bouts antérieurs, et par une faible dispersion des charges répulsifs dans l'espace, et enfin par une légère résistance qu'éprouvaient ces nuages subtils dans l'éther et leur déviation du plan de l'orbite cométaire.

En effet, si l'on calcule les chemins S parcourus par les nuages B et a^1 du 9·68 octobre au 9·68 novembre, premièrement d'après les positions observées et puis d'après les éléments des orbites, on trouve en unités de la distance Terre—Soleil: pour B, S observé est 2·2121 et S calculé 2·4598, d'où le ralentissement du mouvement effective par rapport au mouvement calculé est 0·2477; pour le nuage a^1, S observé est 1·8231, S calculé 2·0455 et le ralentissement 0·2224.

Les vitesses moyennes et les ralentissements (par seconde et en lieues géographiques) des nuages dans l'intervalle de 31 jours seront respectivement: pour B 18·40 et 1·85 et pour a^1 15·30 et 1·66, et l'on a: 1·85 : 18·40 = 0·1005 et 1·66 : 15·30 = 0·1085. D'où il est permis de conclure, que la résistance peut être régardée comme proportionnelle à la vitesse. Pour traiter ce sujet avec plus d'exactitude je veux employer plus tard les formules, que j'ai données dans mon Mémoire sur le milieu résistant (Annales, Vol. VI., 2 livraison).

Les époques trouvées M_1 correspondent probablement aux catastrophes qui ont produit la division du noyau.

La queue du premier type, $1 - \mu = 12$ (hydrogène) est, d'après mes calculs, ce prolongement plus faible et plus étroit de la queue principale qui pour la dernière fois a été observé par M. Cruls (Comptes Rendus, 6 Novembre 1882) le 25 Septembre et qui se projetait en perspective sur la partie antérieure de la queue principale.

§ 6. La formation du tuyau autour de la tête de la comète, observé par M. Schmidt (*l. c.*) et dont les parois sont désignées chez lui par D et D^1, s'explique de la manière suivante, que j'avais toujours employée pour l'explication des queues anomales des comètes.

Les particules du premier type, animées, comme je l'ai montré pour plusieurs comètes, de la vitesse initiale $g = 0·15$, en s'élançant du noyau donnent des chocs aux corpuscules non réduites en molécules ou en atomes et non chargées du tout ou chargées d'une très mince quantité de la force répulsive, par exemple de $1 - \mu = 0{\cdot}015$, et leur communiquent une partie de leur propre vitesse, par exemple $g = 0{\cdot}1$. Ces corpuscules, sorties de la sphère d'action du noyau sous tous les angles avec le rayon vecteur, doivent décrire autour du soleil des ellipses et des hyperboles (des paraboles dans quelques cas particuliers).

En admettant que l'émission des corpuscules a eu lieu vers le 18 Septembre, c'est à dire vers le temps des catastrophes, je calcule rigoureusement pour quelque époque donnée, par exemple pour le 20 Octobre, leurs positions par rapport au noyau, et il en résulte qu'elles se trouvaient sur la surface d'une cloche paraboloïdale entourant la tête. L'ample chevelure de 20′ observée par Cruls (voir le § 1) le 25 Septembre présentait probablement les rudiments de ce tuyau ou de cette cloche, qui a dû avec le temps s'ouvrir à son sommet, du côté du soleil, et devenir ainsi un tuyau.

Voici les résultats numériques de ces calculs effectués à l'aide des formules qu'on trouvera à la fin du Mémoire. Dans leurs constructions graphiques l'echelle est 1 = 300 millim.

(1.) La force $1 - \mu = 0$; $g = 0{\cdot}1$; le temps d'emission des particules M_1 = Sept. 17·58 ; le temps d'observation M = oct. 20·68. Pour l'ellipse l'anomalie excentrique est U ; pour l'hyperbole l'angle correspondant est F. — (Pl. III. fig. 7.)

	G	V_1	U_1	F_1	t	U (F)
No. 1	+ 135°	134° 11′ 53″	—	11° 32′ 19″	33·45560	50° 59′ 54″
2	+ 90	135 11 19	11° 44′ 59″	—	33·45740	62 2 56
3	+ 45	134 54 25	18 6 18	—	33·46104	98 21 28
4	0	133 31 32	17 53 2	—	33·46391	97 33 7
5	— 45	131 51 17	11 20 24	—	33·46455	60 31 50
6	— 135	131 9 10	—	17 29 36	33·45862	66 32 14

	V	lg R	lg Δ	Δ	ϕ	Δ mill.
No. 1	169° 46′ 22″	0·0785659	8·871284	0·07435	32° 3′·1	22·3
2	171 55 50	0·0287648	8·829816	0·06758	168 32·7	20·3
3	173 27 34	9·9873191	9·216493	0·16462	186 26·9	49·4
4	173 13 29	9·9882998	9·221020	0·16635	193 12·4	49·9
5	171 18 24	0·0295286	8·873939	0·07481	209 47·6	22·4
6	167 50 34	0·1080172	9·172137	0·14864	6 40·3	44·6

(2) La force $1 - \mu = 0{\cdot}015$; $g = 0{\cdot}1$; M_1 et M restent les mêmes. — (Pl. III., fig. 8)

	G	V_1	U_1	F	t	U (F)
No. 1	+ 90°	134° 34′ 25″	—	5° 35′ 5″	33·45880	27° 44′ 12″
2	75	134 37 36	6° 56′ 57″	—	33·45988	36 14 25
3	58	134 30 19	10 58 14	—	33·46133	57 46 18
4	45	134 17 12	12 34 49	—	33·46241	66 39 14
5	+ 24	133 44 9	13 30 43	—	33·46405	71 58 19
6	0	132 53 20	12 19 40	—	33·46422	65 35 25
7	— 10	132 30 1	11 3 3	—	33·46575	58 28 29
8	— 45	131 11 18	—	6 25 33	33·46515	31 50 0

	V	lg R	lg Δ	Δ	φ	Δ mill.
No. 1	170° 32′ 43″	0·0584521	8·492713	0·03110	73° 4′·5	9·3
2	171 7 46	0·0438947	8·530538	0·03393	146 55·3	10·2
3	171 39 7	0·0299119	8·803567	0·06362	174 41·2	19·2
4	171 54 59	0·0223442	8·913232	0·08189	182 9·5	24·6
5	172 1 37	0·0170917	8·981303	0·09579	189 2·7	28·7
6	171 38 29	0·0231121	8·922570	0·08367	196 30·1	25·1
7	171 18 9	0·0268619	8·877083	0·07535	199 17·5	22·6
8	169 47 32	0·0601106	8·425017	0·02661	300 38·8	8·0

La ligne S O dans nos figures réprésente le rayon vecteur, et le sens de son mouvement est indiqué par les flèches ; O est le noyau ; O Q est la racine de la queue. Les diamètres des cloches passant par le noyau sont égaux à 0·05, et d'après les données de Schmidt à 0·044.

Les conditions particulières dans le mode de l'expulsion des corpuscules déterminent les particularités dans la structure de la cloche et dans la distribution de lumière à sa surface. Si, par exemple, la vitesse g est plus grande pour les petits angles avec le rayon vecteur, la cloche sera plus allongée ; l'absence de la répulsion rend la cloche beaucoup plus longue pour la même vitesse g etc. . . . Pour évaluer toutes ces conditions diverses il faudrait avoir des observations et des mesures assez exactes.

Pour $g = 0{\cdot}1$ et $1 - \mu = 0{\cdot}015$ le diamètre de la cloche correspond à ses dimensions données par M. Schmidt. L'épaisseur des parois de la cloche ou du tuyau dépend de la durée du temps dans lequel une même vitesse g était en jeu.

Lors des catastrophes mentionnées plus haut la vitesse g fut probablement beaucoup plus considérable qu'à l'ordinaire.

La diminution de g rétrécit en général les dimensions de la cloche. Avec l'evanouissement du I type il n'est resté que la vitesse g plus petite, correspondante à la force $1 - \mu = 2$ et par conséquent l'expulsion des corpuscules non chargées de la force répulsive a dû décroître brusquement, et ces particules ont formé une autre cloche, un autre tuyau plus étroit, que plusieurs observateurs ont vu comme un faible appendice dirigé vers le Soleil.

D'après mes calculs l'angle de cette queue anomale avec le rayon vecteur dans le plan de l'orbite est de 9°, et en perspective 5° − 6°. Il faut ajouter que la queue anomale doit précéder toujours le rayon vecteur, et c'est ce qui a eu lieu dans cette comète, comme on le voit sur les photographies de M. Gill.

L'explication donnée par moi à la formation du tuyau ou de la cloche peut être appliquée aussi à l'apparition de l'objet nébuleux vu par M. Schmidt le 9, 10, et 11 octobre ; mais alors, outre l'impulsion, il faut introduire dans le calcul la résistence que la masse subtile, émise du noyau, au périhélie, vers le Soleil et libre de la force répulsive, a dû éprouver dans la couronne solaire. Je veux prochainement effectuer cette recherche.

Les calculs qui se rapportent aux queues anomales (cloche, tuyau) s'effectuent à l'aide des formules suivantes, dont la déduction n'est point difficile.

Quand la répulsion est nulle, c'est à dire $1 - \mu = 0$, on a pour une particule qui s'elance vers le Soleil sous un angle avec le rayon vecteur $G = 0$:

I. Pour une ellipse.

$$H_1^2 = H^2 + g^2 - 2\,H\,g.\cos.\beta, \qquad \mathrm{Sin}\,\gamma = g.\,\mathrm{Sin}\,\beta : H_1,$$

$$\beta = \beta + \gamma;$$

$$m = H_1^2 \cdot r_1 \quad \text{et}$$

$$1. \quad A = r_1 : (2 - m),$$

$$2. \quad P = m.\,\mathrm{Sin}^2\,\beta_1 \cdot r_1,$$

$$3. \quad E^2 = (A - P) : A,$$

$$4. \quad \cos V_1 = (P - r_1) : r_1\,E,$$

$$5. \quad Q = P : (1 + E).$$

Puis on a :

$$6. \quad \mathrm{tang}\,\tfrac{1}{2}\,V_1 = \sqrt{\frac{1+E}{1-E}} \cdot \mathrm{tang}\,\tfrac{1}{2}\,U_1;$$

$$7. \quad T = \frac{A^{\frac{3}{2}}}{k}\,(U_1 - E.\,\mathrm{Sin}\,U_1)$$

ou k est exprimé en secondes d'arc; c'est à dire $\log k = 3{\cdot}5500066$, U_1 est donné en secondes et E est divisé par Sin 1″.

$$\pi = M_1 - T, \qquad t = M - \pi, \qquad 8. \quad nt = U - E.\,\sin U$$

où $n = k : A^{\frac{3}{2}}$, t est exprimé en jours—

$$9. \quad \mathrm{tang}\,\tfrac{1}{2}\,V = \sqrt{\frac{1+E}{1-E}} \cdot \mathrm{tang}\,\tfrac{1}{2}\,U$$

10. $R = A\,(1 - E.\cos U)$, où E est un nombre abstrait.

Si l'anomalie parabolique du noyau pour l'époque M_1 est v_1, et pour le temps d'observation M elle est v, alors—

$$w = v_1 + (V - V_1),$$

l'angle du rayon vecteur du noyau avec le rayon vecteur de la particule sera $w - v$, et pour la distance Δ entre le noyau et la particule on trouve—

$$\Delta^2 = r^2 + R^2 - 2\,Rr\cos(w - v).$$

En posant $\eta = R.\,\mathrm{Sin}\,(w - v)$ on obtient—

$$\mathrm{Sin}\,\phi = \eta : \Delta$$

où ϕ est l'angle de la ligne noyau-particule avec le rayon vecteur du noyau au moment M.

Dans le cas de $m = 2$ la trajectoire est une *parabole.*

II. Pour une hyperbole.

$$1. \quad A = r_1 : (m - 2), \qquad 2. \quad P = m.\,\mathrm{Sin}^2\,\beta_1\,r_1,$$

$$3. \quad E^2 = (P + A) : A, \qquad 4. \quad \cos V_1 = (P - r_1) : r_1 E$$

$$5. \quad Q = P : (1 + E)$$

$$6. \quad \mathrm{tang}\,\tfrac{1}{2}\,V_1 = \mathrm{tang}\,\tfrac{1}{2}\,F_1 \sqrt{\frac{E+1}{E-1}}$$

$$7.\quad \lambda k \mathrm{T} : b^{\frac{3}{2}} = \lambda\, \mathrm{E}\ \mathrm{tang}\ \mathrm{F}_1 - \log \mathrm{tang}\,(45^\circ + \tfrac{1}{2}\mathrm{F}_1)$$

$$\log \lambda = 9{\cdot}6377843 - 10,\ \log k = 8{\cdot}2355814 - 10$$

Où—

$$b = \mathrm{P.}\ \mathrm{cotg}^2\,\phi\,;\qquad \cos\phi = 1 : \mathrm{E}$$

$$\pi = \mathrm{M}_1 - \mathrm{T},\qquad t = \mathrm{M} - \pi.$$

$$8.\quad \lambda k t : b^{\frac{3}{2}} = \lambda\, \mathrm{E}\ \mathrm{tang}\ \mathrm{F} - \log.\ \mathrm{tang}\,(45^\circ + \tfrac{1}{2}\mathrm{F})$$

$$9.\quad \mathrm{tang}\ \tfrac{1}{2}\mathrm{V} = \mathrm{tang}\ \tfrac{1}{2}\mathrm{F}\ \sqrt{\frac{\mathrm{E}+1}{\mathrm{E}-1}}$$

$$10.\quad \mathrm{R} = \mathrm{P} : (1 + \mathrm{E}\cos \mathrm{V})$$

Si l'émission fait un angle G avec le rayon vecteur (où G est positif quand l'émission est à l'arrière du rayon vecteur), on a—

$$\mathrm{H}_1{}^2 = \mathrm{H}^2 + g^2 - 2\,\mathrm{H}g\cos(\beta - \mathrm{G})$$

$$\mathrm{Sin}\,\gamma = g.\ \mathrm{Sin}\,(\beta - \mathrm{G}) : \mathrm{H}_1,\quad \beta_1 = \beta + \gamma,\quad m = \mathrm{H}_1{}^2\, r_1.$$

En introduisant une répulsion $1 - \mu$ on aura $\mathrm{m} = \mathrm{H}_1{}^2 \cdot r_1 : \mu$ et au lieu de k on doit prendre $\mathrm{K} = k\sqrt{\mu}$.

Les lettres dans les formules précédentes ont les significations suivantes :—

H—la vitesse du noyau (pour l'unité de temps égale à 58·13244 jours) dans la direction de la tangente, pour l'époque de l'émission M_1.

r_1—le rayon vecteur du noyau pour cette époque.

β—l'angle de ce rayon avec la tangente ; il est aigu après le périhélie du noyau.

V_1—l'angle entre le rayon vecteur r_1 et l'axe de orbite elliptique ou hyperbolique de la particule ; cet angle est *négatif avant* le passage du noyau à son périhélie.

Q—la distance périhélie,

P—le demi-paramètre,

E—l'excentricité,

A—le démi-grand axe de l'ellipse ou le demi-axe transversale de l'hyperbole.

T—l'intervalle de temps entre l'émission de la particule du noyau et son passage au périhélie de son orbite ; T est positif ou négatif—selon le signe de V_1.

π—le temps du passage de la particule au périhélie de son orbite, il est égal à M_1—T.

t—l'intervalle entre le passage de la particule au périhélie de son orbite et le moment de l'observation M ; il est égal à $\mathrm{M} - \pi$. Tous les moments de temps sont exprimés en jours.

V—l'anomalie vraie de la particule pour l'époque M,

R—le rayon vecteur de la particule à cette même époque M.

w—l'angle entre le rayon R et l'axe de l'orbite du noyau.

Si l'orbite du noyau est une parabole, $\mathrm{H}^2 = 2 : r_1$ et $\beta = 90^\circ - \frac{1}{2}v_1$.

Les anomalies vraies sont comptées positives après les passages aux périhélies.

Th. Bredichin.

1883, 14 avril.

THE CHROMOSPHERE IN 1882.

By the Rev S. J. PERRY.

The observations taken during the past year indicate very clearly the approximate time of the maximum disturbance in this portion of the solar atmosphere. Only those observations are included in the mean results, where the chromosphere has been accurately measured throughout the whole circumference.

	Number of days of Observation.	Mean height of Chromosphere, excluding Prominences.	Mean extent of Prominence arc.	Mean height of Prominences.	Highest Prominences.
January,	1	7″·7	27° 18′ 0″	22″·2	52″·5
February,	1	7·7	32 17 0	23·9	46·1
March,	7	7·8	33 41 34	25·7	93·1
April,	10	8·6	56 13 24	28·7	145·2
May,	17	8·4	48 58 56	26·5	85·4
June,	6	8·5	36 4 10	22·4	55·5
July,	7	8·6	37 49 0	22·3	72·5
August,	5	8·5	41 3 48	27·2	93·9
September,	9	8·2	38 59 7	22·2	71·7
October,	2	8·3	51 11 0	24·8	63·2
November,	2	8·5	48 0 30	23·9	57·6
December,	2	8·1	39 45 0	24·8	52·5
1882,	5·8	8·24	40 56 47	24·55	74·10
1881,	4·3	8·04	33 18 26	24·61	71·93
1880,	5·3	7·93	23 21 23	23·46	67·40

Comparing together the results of the last three years we notice at once a steady increase in the average height of the chromosphere and in the extent of the prominence arc, but the mean height of the prominences observed has remained nearly constant during the last two years.

The prominences were more than 1′ of arc in height on three days in March, on 6 in April, on 10 in May, and once in July, August, September, and October; and twice in April, viz., on the 8th and 9th, the height exceeded 2′ of arc. The maximum of 65340 miles observed on the 8th is the second greatest prominence ever observed at Stonyhurst.

The months of April and May were very exceptional, more on account of the contortions of the chromospheric lines, than of the heights of the prominences. From the 6th to the 10th of April, and from the 10th to the 14th of May, daily observations were obtained, and on each day there were strong indications of solar storms. Similar evidence was furnished by almost every observation of April, and by more than half the observations in May. During the following months the same phenomena were not at all unfrequent.

On three occasions, viz., on April 6th, May 31st, and November 3rd, the bright C line gave decided proof of cyclonic motion in the chromosphere, as it was clearly visible at both sides of the dark Frauenhofer line.

On September the 16th, the surface of the chromosphere was remarkably uneven throughout, and this does not appear to have been due to haze.

Stonyhurst College Observatory,
May 8th, 1883.

NOTE ON THE ACCURACY OF LALANDE'S RIGHT ASCENSIONS.

By J. L. E. DREYER.

In No. 657 of the Astronomische Nachrichten Dr. Lindhagen communicated the results of an investigation on the accuracy of the star-places of Lalande as given in Baily's Catalogue. He finds that the probable errors of one observation are fairly well represented by the following formulæ—

$$r(\alpha) = \pm \sqrt{(0^s{\cdot}1233)^2 + (0^s{\cdot}1725)^2 \sec^2 \delta}$$
$$r(\delta) = \pm \sqrt{(2''{\cdot}013)^2 + (0{\cdot}008563)^2\, r^2}$$

where r is the Mean Refraction expressed in seconds.

I have already on a former occasion mentioned that whenever a star occurs more than once in the Histoire Céleste, the single Right Ascensions as computed by von Asten's tables almost always agree better *inter se* than the corresponding Right Ascensions in Baily's Catalogue do. Having lately heard from a French Astronomer, M. Bossert, that he had computed a number of zones by von Asten's tables, but that he had not always found my experience confirmed, I have looked a little more closely into the matter. From the number of Right Ascensions of stars between ± 15° Declination which I had computed for determining the constant of Precession, I picked out at random three hundred, belonging to a hundred and fifty stars, each star having been observed twice. The simplest way to find the probable error of an observation in a case like this is to take the sum of all the single differences (Δ) of the 150 pairs, we have then—

$$\text{Prob. error of one obs.} = 0{\cdot}8453 \frac{\Sigma\Delta}{150\sqrt{2}}.$$

The result found in this way was $\pm\, 0^s{\cdot}1205$. There are a number of stars in the Histoire Céleste which were observed three or more times, but as these are nearly all bright stars, I preferred to treat them separately. 120 observations of 34 stars gave the probable error of one observation = $\pm\, 0^s{\cdot}1219$. Lindhagen's formula gives between ± 15° Declination $\pm\, 0^s{\cdot}2137$, so that it is very evident, how imperfectly the tables of Hansen and Nissen (on which Baily's Catalogue is founded) do justice to Michel Lalande's and Burckhardt's excellent observations. For the sake of comparison it may be mentioned here that the probable error of one RA in Weisse's and Schjellerup's Catalogues between ± 15° Decl. is respectively $\pm\, 0^s{\cdot}1566$ and $\pm\, 0^s{\cdot}084$. It is very likely that a renewed reduction of Bessel's zones would diminish the probable error found by Weisse.

I regret not having access at present to a copy of the Histoire Céleste, so that I am not able to extend the above examination to the Declinations.

ON THE MULTIPLE TAIL OF THE GREAT COMET OF 1744.

BY J. L. E. DREYER.

Up to about twenty years ago the observation by Loys de Chéseaux of the sixfold tail of the splendid Comet of 1744, on the mornings of the 7th and 8th of March, was the only testimony astronomers possessed of this very remarkable fact. As apparently none of the other observers had noticed the phenomenon, not even Heinsius, who has given a full description of the physical appearance of the comet before the perihelion passage, some people have been inclined to doubt the trustworthiness of Chéseaux's observation, particularly as Heinsius mentions that he looked for the comet shortly after four o'clock on the morning of March 7, but saw nothing in the east except "bei schon etwas merklicher Dämmerung ein Stück eines Nordscheines mit häufig aufsteigenden kurzen Säulen."[*] It should however be mentioned, that the head of the comet after the perihelion passage (March 1) for an observer in our latitudes would be below the horizon until a short time after sunrise, so that most observers no doubt never took the trouble to look for the comet after the end of February.

In 1864 Professor Winnecke published in the Bulletin of the Academy of St. Petersburg, a paper "Ueber den vielfachen Schweif des grossen Cometen von 1744" (Mélanges math. et astron. T. III. p. 503). in which he gave extracts from De L'Isle's unpublished observations of the comet. These show that De L'Isle on the mornings of the 6th and 7th of March saw several tails. On the 6th he saw "plusieurs traits lumineux en forme de queues de Comète qui se levoient au dessus de l'horison, dirigés de la bouche du Pegase et du petit Cheval vers le Cigne, ils avoient 15 à 20 degrez de longueur." On the 7th he saw four tails of which the best defined one started from immediately preceding ϵ Pegasi and pointed towards κ Pegasi; it was seen for more than an hour. The other three tails were following and parallel to the principal one. On the following mornings the sky was overcast, and on the 18th there was no trace of tails to be seen. Counting from East to West the two first of De L'Isle's tails would be too close to the horizon at Lausanne and were therefore not seen by Chéseaux, while the fifth and sixth tail seen by the latter for the same reason were not noticed at St. Petersburg. The third and fourth of De L'Isle's tails correspond exactly with the first and second seen by Chéseaux, while it remains unexplained why De L'Isle did not notice the third and fourth tail seen at Lausanne.

In Vol. VII., Part 1, of the Annales de l'Observatoire de Moscou Professor Bredichin has discussed all the physical observations of the Comet of 1744, including the observations of several tails by Chéseaux and De L'Isle. He finds that the *positions* of these tails can be represented by repulsive forces comprised between 1·0 and 2·4, and that the general axis of this tail, composed of several conoïds, corresponds to the mean value $1 - \mu = 1{\cdot}7$ (second type). But there is this difficulty, that the *directions* of the tails as drawn by Chéseaux (who joins the tails to the Nucleus below the horizon by dotted lines) give the tails far too little curvature and disagree both with the theory and with his drawing of February 28. On the other hand, the directions of the tails seen by De L'Isle agree much better with the theory, and as Chéseaux's drawings (as remarked by Winnecke) do not agree perfectly with his text on all points, too much stress should perhaps not be laid on the direction of such ill-defined and diffused nebulosities as the tails must have been.

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[*] Beschreibung des im Anfang des Jahrs 1744 erschienenen Cometen, von G. Heinsius, St. Petersburg. 1744, 4to, p. 18. As Chéseaux's account is quoted in Winnecke's paper on the Comet of 1858, in his paper on the Comet of 1744, and in the Annales de l'Observatoire de Moscou, T. VII., Livr. I., it is unnecessary to repeat it here.

Among a number of old pamphlets and books on remarkable comets, which I have from time to time collected, is an anonymous little book entitled: "Beantwortung verschiedener Fragen über die Beschaffenheit, Bewegung und Würkung der Cometen. Berlin, zu finden bei Ambrosius Haude, 1744," in two parts, the second being called "Fortgesetzte Beantwortung &c.," 56 and 92 pp. 8vo, with three plates. In Weidler's and Lalande's Bibliographies no Author is mentioned to this book, but in the Pulkova Library Catalogue and in Poggendorff's Biographical Dictionary the authorship is attributed to Leonhard Euler. It is curious, that nobody seems to have noticed the valuable confirmation of the observation of Chéseaux contained in this book, the more so as two of the plates represent the appearance of the multiple tail of the comet on the 5th and 7th of March. Most of the book is taken up by a discussion of the motion and nature of comets, with an account of the motion of the comet of 1744. Then follows (in the second part) a chapter containing observations of this comet made "von einem geschickten Frauenzimmer," describing first the comet's motion among the stars from January 3 to February 25, after which comes an account of the tail, headed: "Ueber den Schweif des Cometen sind über dieses Noch folgende Betrachtungen gemacht worden" and filling pp. 85–92. This account is evidently also written by the "clever woman" who wrote the beginning of the chapter. Deferring for the present the question as to who she can have been, I now reprint that part of the chapter which refers to the tails seen after the perihelion passage (pp. 88–92.)

"Es war auch nicht möglich, sich wegen des trüben und gewölckten Himmels eher wieder nach dem Cometen umzusehen, bis den 5. Martii des Morgens. Der Himmel war zwar anfänglich noch ziemlich bewölckt, es blickten aber doch früh um 4. Uhr einige Sterne, und gegen Osten wurde man durch die ziemlich dicke Luft einige Strahlen gewahr, welche etwas schief nach Norden zu etwa 10. Grad hoch in die Höhe giengen. Es schien, als wenn etliche Cometen neben einander stünden, welche am Horizont von Dünsten bedecket würden, daher es kein Wunder, dass einige Leute gesagt, sie hatten 3. oder auch wohl 4. Cometen des Morgens gesehen; denn diese Strahlen sahen nicht viel anders aus. Man hatte aber Ursache zu vermuthen, dass der einige Comet seinen Schweif so ausgebreitet, und dass der Cörper selbst hinter dem Horizont seyn, und bald hervor kommen würde. Als man sich hernach auch an dem gantzen Himmel umsahe, so erblickte man gegen Nord-Ost zwischen zertheilten Wolcken einen

Nordschein, welcher gelb und helle war, auch liessen sich Strahlen darinnen sehen, welche sich mit einer mercklichen Geschwindigkeit gegen die Cometen Strahlen zu bewegten, aber auch schnell wieder zurück giengen, dergleichen schnelle Bewegung bey denen Nordscheinen öfters geschiehet. Jedoch sahe dieser Anblick gar fürchterlich aus, denn der Cometen Schweif war so ungemein ausgebreitet, und der Nordschein schien den gantzen Himmel in Bewegung zu setzen. Man kam deswegen fast auf die Gedancken, dass die Oestlichen Strahlen auch vom Nordschein herrühreten, und also gar nichts mehr vom Cometen vorhanden wäre; allein nach einiger Betrachtung vergieng der Nordschein, und die Strahlen gegen Osten blieben beständig stehen, sie erhoben sich aber mit den Sternen, also dass da sie erst etwa 10. Grad hoch über den Horizont hervor giengen, sie eine Stunde hernach auf 20. Grad lang zu sehen waren.

Sie sahen auch blasser und trüber aus, als die Nordscheins-Strahlen, und behielten immer einerley Gestalt und Figur. Wenn man eine Vergleichung machen sollte, so könnte man sich die Figur dieser Strahlen als einen grossen ausgebreiteten Flügel vorstellen, welcher hinter dem Horizont herauf stieg, dessen Länge um 5. Uhr etwa 20. Grad, und die Breite auch fast so viel betrug. Denn je höher er kam, desto mehr Strahlen kamen auch südlich in die Höhe, bis sie durch des Tages Anbruch blasser wurden, und hernach nebst denen Sternen endlich gar verschwunden. Man hoffte zwar den Cometen selber noch vor Sonnen-Aufgang zu erblicken, aber so wohl die Morgenröthe, als auch die dicke Luft, welche den Horizont einnahm, hinderten, dass man weiter nichts sehen konnte.

Hierauf erwartete man mit Verlangen einen hellen Morgen. Den 6. Mart. war es trübe, aber den 7. Mart. früh war der Himmel hell. Um 4. zeigten sich die Strahlen gegen Osten wiederum, wie den 5. Martii. Sie stunden wie grosse Balcken etwa 15. Grad vom Horizont in die Höhe, etwas schief gegen Norden zu gelencket. Der nördlichste Strahl gieng nahe vor dem Stern *Scheat* südlich vorbey, von da an giengen die übrigen Strahlen vom Ost-Horizont neben einander bis unter den *Delphin*, sie erreichten aber denselben nicht völlig. Mitten in den Strahlen blickte der Stern, welcher auf dem Maule des *Pegasi* zu finden, der in Bayero mit ε in der Doppelmayerschen Charte aber mit α bezeichnet ist. Zwey Tage zuvor, nemlich den 5. Mart. stund dieser Stern gantz nahe über den Strahlen, diesen Morgen aber meist mitten drinne: also sahe man, dass sich die Strahlen verlängert und auch noch mehr ausgebreitet hatten. Sie stiessen nicht gantz bis an den Horizont, denn es war daselbst eine blasse Helle, welche durch des Tages Anbruch noch mehr zunahm, bis endlich die Strahlen wegen Annäherung des Tages nicht mehr konnten gesehen werden. Man gab Achtung, bis die Sonne aufgieng, ob der Comet selbst noch zum Vorschein kommen möchte, aber es war vergebens.

Den 8. Martii früh war trüber Himmel.

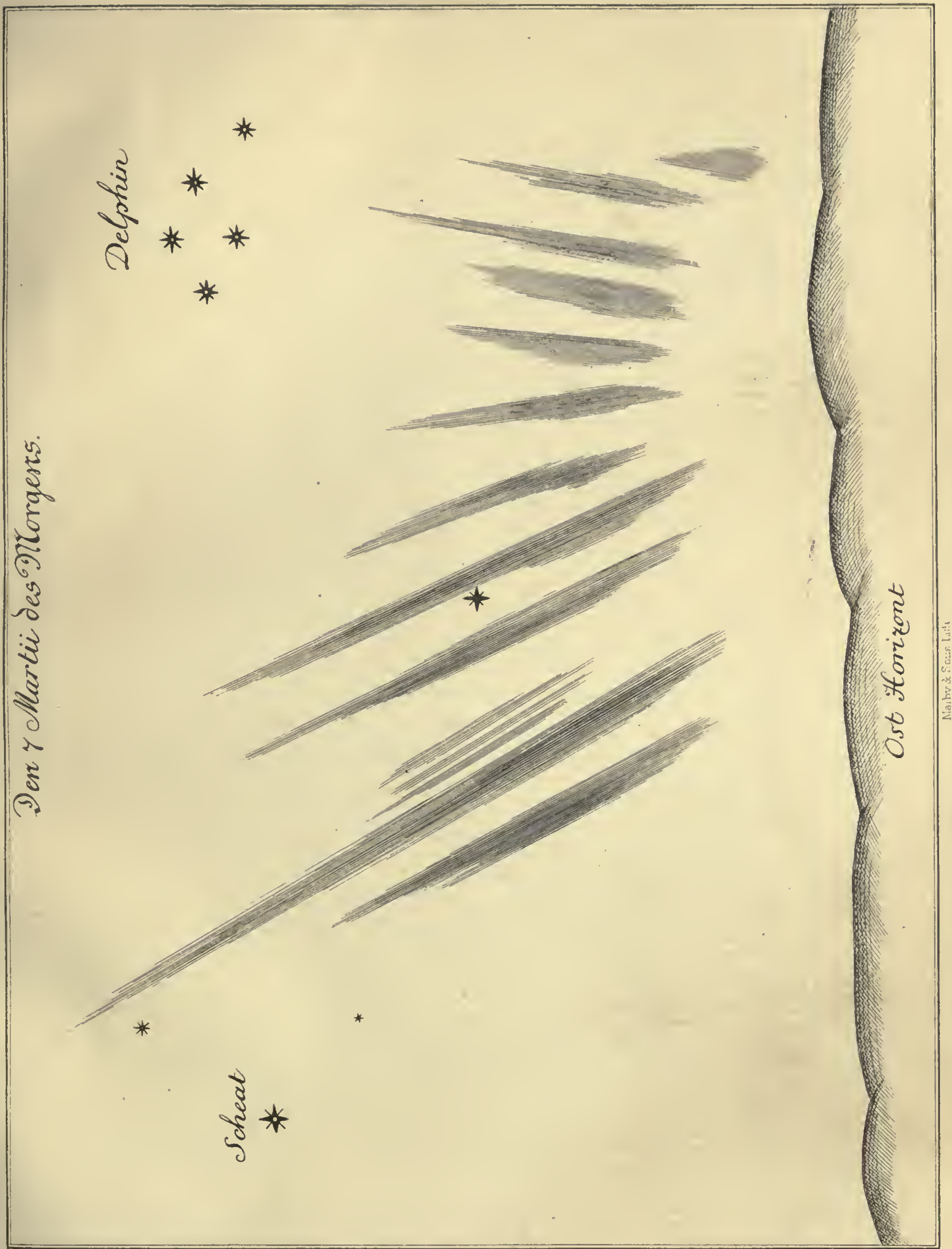
Den 9. Martii früh um 4. Uhr war der Himmel meist hell, die Strahlen zeigten sich wieder im Osten, wie den 7. Martii, erstreckten sich aber noch etwas höher. Die südlichen Strahlen, welche allezeit etwas kürtzer als die Nördlichen, hatten nunmehr mit ihrer Länge den *Delphin* vollkommen erreichet.

Den 10. Martii früh sahen die Strahlen noch so aus, wie vorigen Morgen, nur etwas blasser.

Den 11. Martii war die Nacht trübe und ein hefftiger Sturm-Wind.

Den 12. Mart. war der Himmel des Morgens klar. Gegen 5. Uhr stunden die Strahlen ziemlich hoch, sie sahen aber ganz blass aus, nicht heller als die Milchstrasse.

On the second and third plate of the book in question are shown the "rays" seen on the mornings of the 5th and 7th of March. The only star shown on the second plate is ϵ Pegasi, below and to the right of which are twelve or thirteen short rays, which, if prolonged, would meet at a point only a few degrees below the horizon (in reality the head of the comet was at the time about 20° below the horizon). The third plate is reproduced in plate IV. accompanying this paper. The stars shown are Sheat or β Pegasi with η and μ Pegasi to the right of it; in the midst of the rays is ϵ Pegasi and higher up are the five principal stars of Delphinus



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A comparison with the drawings of Cheseaux and De L'Isle as copied in Winnecke's paper shows that the rays below Delphinus are identical with the third and fourth of the tails seen at Lausanne, while the Berlin observer like De L'Isle did not see the fifth and sixth. The rays immediately to the left and right of ϵ Pegasi are De L'Isle's third and fourth and Cheseaux's first and second tail, while the two most northerly rays, which on the fifth of March were very short, are probably the same as De L'Isle's first and second tail. The identification is however somewhat uncertain, as the Berlin observer draws far more rays than the two other observers do and as he has inserted so very few stars on his drawing. Probably he only intended to give a general idea of the appearance of the rays but did not pretend to any great accuracy as to details.

As to the discrepancy between theory and observation, pointed out by Professor Bredichin, it deserves notice, that the Berlin drawings agree fairly well with the theory except perhaps so far as the two or three most southerly rays are concerned, which ought to be nearly in the direction from north to south according to the theory. It is however a favourable circumstance to Professor Bredichin's views, that the two southern tails seen at Lausanne, which differ most as to direction from what is indicated by theory, were neither seen at St. Petersburg nor at Berlin, and as they were very close to the horizon and were very broad and short, their direction might easily be mistaken.

The weight to be attributed to the description reprinted above of course mainly depends on the answer to the question: who is the "geschickte Frauenzimmer"? It appeared to me at once to be likely that she was Christine Kirch or one of her two sisters, daughters of Gottfried and Maria Margaretha Kirch. The sisters seem to have lived with their brother Christfried Kirch, who in 1716 was appointed Astronomer to the Berlin Academy, until his death on March 9, 1740. Christine lived till 1782; about her sisters I cannot find any information. Several members of this family kept for about half a century (beginning, I believe, with 1697) a series of meteorological diaries containing also many astronomical observations of value.* Having enquired from Mr. J. G. Lohse whether these diaries, which are now in the splendid astronomical library at Dun Echt, contain any observations of the tails of the comet of 1744, Mr. Lohse very kindly sent me a copy of all the notes in the diary for 1744 describing the appearance of the comet after the perihelion passage. Lord Crawford has kindly permitted me to publish these notes, and considering the interest attached to the subject, I have thought it desirable to give them in extenso, so that anyone may judge for himself as to whether the description in Euler's little book is written by the same individual who wrote the diary, or not. I may add, that the description in the diary of the appearance and motion of the comet during January and February often contain the same expressions as the printed account, though it is as a rule much fuller.

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* See *e.g.* my Supplementary Catalogue of Nebulæ, p. 397.

The diary for 1744 is headed: "Gewitter Observationes 1744. *M.K.*" On a loose sheet in one of the diaries it is stated that at the time of Maria Margaretha Kirch's death (1720) three daughters and one son were living (according to Jöcher, Gottfried Kirch had fourteen children), and that the second daughter married. The writer, M.K., must therefore have been the third daughter; she mentions her sister Christine, who is better known to astronomers, as C.K. (see below under March 7). "Der junge Grischo," whom she often mentions, must be Augustin Nathanael Grischow, born 1726, died 1760 as Professor in St. Petersburg. His father, Augustin Grischow, seems to have been an assistant at the Berlin Observatory, where he was himself Astronomer from 1749 to 1751 (see R. Wolf's Geschichte der Astronomie pp. 459 and 639). Who *J.A.* is, I cannot make out.

It will be remarked that the notes are very hastily written and with no attention to spelling and punctuation. The writer's mind seems to have been totally engrossed by the remarkable phenomena she witnessed, as she from March 5 omitted to record her usual readings of Barometer and Thermometer:—

MARTIUS 1744.

5. Weil abends zuvor der Himmel helle war so resolvierten wir diesen Morgen früh aufzustehen und den Cometen zu suchen, den der Comet seynen lauf proportionirlich fort gesetzet, so würde er etwa gegen 5 Uhr aufgehen müssen und alsden sichtbar sein, wen er gleich mit der Sonnen ☌ gehalten, den weil man nicht hat observiren können, ob der Comet Südlich oder Nördlich gehet, so schlosz das er nach gerade etwas Südlich ginge, er doch noch auf 1½ St. vor ⊙ aufgang aufgehen könte, die Nacht aber ward es trübe. Um 4 Uhr sahe zum Fenster herausz so war es trübe, doch blickte ein par Sterne durch die dicke Luft, darauf ward von den jungen Grischo gerufen, woh der Comet stünde, were es helle, und das Dienst-Mädchen schrie auf der Straszen sie sahe den Strahl. Ich zog mich an und ging herunter da war es ein Virte auf 5 Uhr, wie herunter kam, kam mir der junge Mons. Grischo entgegen gelaufen und sagte es weren 4 Cometen am Himmel. Wie ich auf die Strasze kam so däuchte mir über al trübe, am Ost Horizond sahe ungefehr 4 Strahlen durch die dicke Luft blicken, welche aber ziemlich nahe bey ein ander waren, daher schlosz das der Cometen Schweif sich so sehr aus breittete theile und etliche strahlen von sich würfe, nur wunderte mich da es doch trübe schien zu sein und ich in die Gegend keinen Stern sahe man doch die Strahlen sehen konte, der Mond war gegen den Mittag gantz mit Wolcken bedeckt das mir also der Himmel gantz trübe vor kam, wie ich aufs Observatorium kam so war gegen Nord Ost eine Wolckenspalte da sahe eine schöne hellichkeit von Nordschein und war auch Bewegungen und Strahlen zu sehen und bewegete sich einen Augenblick bis zu denen Strahlen die vor den Cometen strahl gehalten, daher meinete ich hätte geirrt, und waren Strahlen von Nordschein den es sah fast so aus nur das es ein wenig trüber und nicht so gläntzent schien zu sein als die Nordschein Strahlen, gedacht ob etwa der Himmel da was dicker were und es davon käme, aber die Nordschein Strahlen verlohren sich in augenblick und war nur zuweillen eine gelbligte helichkeit bis sich die den auch verlohr, aber die Strahle blieben beständig gegen Osten und wie der Himmel sich dordhin ein wenig aufklärte so wurd noch mehr Strahlen gewahr, ein Strahl den ich vor den Haupt Strahl hielt war nach einen Stern gerichtet und bedeckte bey nahe denselben, der war gegen Osten. Weil es so wolckigt war und ich den zusamenhanck der Sterne nicht sehen konte, so kan nicht wissen was vor ein ∗ es war, Herr Schuhmacher meinete das es Scheat wer der junge Grischo will das der helzte im Kopf des Pegasie sey.* Diese Strahlen kamen mit den Stern mehr und mehr in die höhe des Tages anbruch verhinderte das so wohl die Sterne

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* It must have been ε Pegasi.

als die Strahlen verschwande gegen halb 6 Uhr waren fast nichts von Strahlen zu mercken es war so beraus roth und herauchicht, so ging um 6 Uhr wieder zu Hause die Wolcken waren auch sehr roth die am himel war.

Der junge Grischo meinete es hätte ihm gedäucht das sie kurtz vor Sonnen Aufgang nicht weit von der Sonne Südlich etwas durch die Wolcken spalte etwas erblickt das hatte wie ein halber Mond geschienen, aber doch kan er nicht gewis.

Ich gestehe ich bin in groszer werwunderung gesetet über die fiellen Strahlen, und das den Cometen nicht finden konte, da anfanglich nicht recht glaubte das die Strahlen von Cometen schweif weren, so hofte, der Comet würde noch auf gehen, und sein schweif würde so schmahl erscheinen wie ihn den 24 Febr. gesehen, oder nur etwas gröszer, und weil der Kopf doch fiel Kendlicher als der schweif, so würde man doch etwas sehen, das bey nahe in gedanken möchte kommen ob der Comet weil er der Sonne zu nahe gekommen er sich zertheiltet (wie unterschiedene Alten gemeinet) auf gelöset worden und in die Strahlen verwandelt.

Den 6ten trübe.

7. Früh um 3 Uhr ward von Grischos Mädchen gewecket, weil es aber noch so früh war legte mich noch nieder bis 4 Uhr, es war gantz heller Himmel, wie um 4 Uhr zum Fenster heraus sahe so war am Ost Horizond etwas dicke, doch merckte hinter Weideman's Hause eine heligkeit schimmern, vermühte das von Cometen Strahl herkäme, weil nicht das gantze Ostliche theil sehen konte so ging herunter auf die Strasze so sahe einige Strahlen gegen Ost Nord Ost da die Luft gantz Reine war, die wie Balcken stunden, etwa 15 Grad über den Horizond, sie gingen etwas schiewlich vom Horizond Nördlich in die Höhe und die Strahlen die Südlich wahren gingen gegen den Horizont etwas Nördlich doch nicht so mercklich als die Strahlen die Nördlich waren das also die Strahlen gegen den Horizond sich einigermahszen conzentrirten und überwärtz sich ausbreiteten, an Horizont war es zu helle das die Strahlen nahe an denselben sich verlohren und also keine streiffen sondern nur eine blasze heligkeit sahe, dieses zusamen mus also der Sweif von Cometen sein und sein Kopf ist vermutlich so nahe bey der Sonne das man ihn also nicht sehen kan. Die Nördlich seite oder Strahl ging bis an den Scheat und denen Sternen die in den bein des Pegasie vorbey, etwas höer als Scheat, und das Südliche theil oder die Strahlen die gingen bis unter den Delvin, die selben waren nicht so lang als die Nördliche Strahlen und errichten den Delvien nicht, wen die Strahlen über haupt nehme, so ist die breite des Sweifes bey nahe 30 Grad breit, ohngefehr mitten im Schweif war ein heller Stern zwischen den Scheat und Delvin doch näher am Horizond, ist vermutlich der Stern den wir vor gestern sahen der dazumahl an den einen Strahl rürete, er ist der Stern der in den Kopf des Pegasie ist, auf dem Maule stehet, es scheinet also als wen der Schweif länger were als vorgestern doch kan nicht wissen weil dazumahl der Himel so trüb wolckit war, diese Strahlen gleichen den Nordscheins Strahlen, an farbe wohl etwas doch sehen die Strahlen von Cometen Schweif etwas trüber und nicht so deudlich *ob gleich heller Himmel war* da die Nordscheins Strahlen ins gemein gläntzen und deutlich seyn, doch es kan wohl seyn das des Tages anbruch verhinderte* das die Strahlen blasz waren. Diese betrachtung habe auf den observatorium gehabt, da mit C.K. oben gewesen. Ich ging gegen Sonnen aufgang wieder, aufs observatorium, da vohrher ein wenig herunter gewesen und habe mit andern mit aller mühe mich umgesehen ob etwas von Cometen Kopf erblicken könte aber es war alles vergebens.

8. War trübe.

9. Früh bald nach 4 Uhr sahe J. A. zum Fenster heraus da war es meist heller Himmel sie sagte das der Strahlen zu sehen were darauf sahe auch zum Fenster heraus und merckete dieses das die Strahlen bis an den Delvien Reicheten, des Tages anbruch verhinderte bald das man die Strahlen nicht recht deudlich sehen könte wie ich mich etwas umgesehen hatte, so legte mich wieder zu Bette. Der junge Grischo ist aufs observatorium um 5 Uhr gewesen da sind die Strahlen was blass gewesen. Diesen Nachmittag war sehr unfreundlich wetter wind Trüb Schneeicht, Abends auch trübe das wir nicht glaubten folgenden Morgen was zu sehen. Aber des Abends blicken doch die Sterne.

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* = bewirkte.

Den 10ten. Diesen Morgen sehe wieder zum Fenster heraus um 4 Uhr die Strahlen sahen was blasz aus der Himmel war gantz gestirnt aber doch war die luft nicht gantz reine, sonst habe keine sonderliche verenderung seid gestern gemercket.

Den 12. Diesen Morgen sahe J. A. gegen 4 Uhr zum Fenster heraus da sie vorher auch schon etliche mahl heraus gesehen konte aber nichts von Strahlen sehen, ich sahe auch zum Fenster heraus und sahe den Delvin wohl hinter Weidemand's her vor kommen konte aber nicht von strahlen sehen eine blasze weiszlichkeit mercke wohl aber weil keine stralen sah so legte mich nieder. Der Kleine Grischo aber Klopfte an und sagte wir konte die strahle hier nicht sehen, so ging mit J. A. aufs observatorium, wie wir oben kamen sahe anfanglich nichts wie aber recht achtung gaben so waren die strahlen gar blasz sie erstrecken sich sehr hoch da sieh 2 Strahlen zum deudlichsten sehen liszen sie hatten fast eben die figur vorige Morgen nur das sie länger und blaszer waren der Südliche Strahl oder mir zur rechten war, der war etwas kürtzer als der Nördliche der mir also zur lincken stand, der Südliche Strahl ging zimlich gerade in die höhe ging zwischen den Delvin und den Adler in die höhe schätze ohngefehr beynahe 40 grad von horizond (die Zeit war gegen 5 Uhr) der Nord strahl ging schiewlich gegen den Nord polo in die höhe ich merckte mir ein paar Sterne da der strahl hin traf so deucht mir das die selben im gürtel des Ceveus ist, den der Strahl ging durch die Milchstrasze zwischen den helen Stern im schwantze des Schwahnes und der Cassiopea herüber, schatz also die länge auf 60 grad, und die breite der biden strahlen von eingut über 30 grad breit.

13. Diesen Morgen bin nicht auf gewesen. Herr Professor Grischo mit seinen Sohn ist aufs observatorium gewesen der sagt das der Strahl der über den Delvien gegangen ziemlich deudlich gewesen. Weil der lange blasze Strahl bis an den Ceveus ging so vermuthete des abends denselben zu sehen es geschahe auch aber es war sehr blas und nicht heller als die Milch Strasze nur das war bedencklich das nahe am horizond der Strahl nicht zu sehen war sondern von der Milch Strasze war er nur Deudlich und ging über den Kopf des Ceveus bis an das Cinglium weil also der Strahl von der Milch Strasze ab gin so meinete Herr Grischo ob es nicht ein Arm von der Milchstrasze were aber ich finde nicht in den Karten woh die Milch Strasze an gezeichnet ist das so ein Strich bis an das Cinglium des Ceveus ginge, ich kan nicht anders vermuhten als das es von Cometen ein Strahl is.

14. Abends habe den Strahl wieder gesehen wie 13.

15. Weil es ⊙t. war so wolte nicht Früh auf stehen. Abends war es trübe.

16. Weil nicht hoffnung hatte das Früh würde helle sein bin ich nicht früh auf gestanden. Herr Grischo mit seinen Sohn ist auf gewesen haben aber nichts mehr von strahlen gesehen. Abends sahe ich um 9 Uhr gegen Norden da sahe noch den Strahl der bis an das Cingelium des Zeweus richte.

17. Abends der Himel ward hell und habe noch den Strahl gesehen aber er war sehr blasz.

18. Abends meist heller Himel der Mond schien also war die Milch Strasze schwach zu sehen mir deucht als wen noch den Stral gemercket häte.

19. War wolcken.

20. War noch mehr gewolcket.

Die Zeit nachher ist des abends gewölck gewesen, den 25ten war der Himmel wohl ohne Wolcken aber Mond schien so helle das die Milchstrasze nicht sehen konte so war der strahl eben falds nicht zu mercken.

Den 2ten Martius.* Abends merckte bey zeiten eine blasze helligkeit als von Nordschein es waren aber auch wolcken ich wartete bis es recht finster ward ob noch den Strahl sehen würde, aber es formirte sich ein Nordscheins Bogen der bis an die schuldern des Ceveus ging und zu weillen noch etwas drüber es war auch eine kleine bewegung drinen ich konte den Strahl nicht sehen aber die helligkeit war schuld das nicht was gewiszes sagen kan, der Nordschein bedeckte die Milchstrasze um 10 Uhr kamen dunckle Wolcken und bedecken den gantzen Himmel.

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* Should be April.

APRILIS 1744.

4. Diesen abend war wie micht deucht heller Himmel ich habe nicht gleich aufgeschrieben ich weis also nicht ob es diesen Abend oder folgender Abend war, da noch den Strahl gesehen, indem ich aufschreibe so fellet mir ein das beyde Abend gesehen habe.

Den 6 bin ich nemlich M.K. mit den Jungen Mons. Grischo aufs Observatorium gewesen da (es war 2 Uhr) habe den Strahl noch gesehen die Strahlen die die Südwärds waren die waren gantz weg, also mus der Strahl von der Milchstrasze seyen also müssen wohl den 12ten Marts. die Strahlen die zwischen den Delvin und den Adler gegangen seyin die Nördlichen Strahlen gewesen seyn und also der Comet Südlicher gegangen seyn.

Wie wir uns um sahen so war ein schmahler Streiff gegen Norden etwa über 10 grad von Horizond und denselben paralel, derselbe erschien und verschwand bey einer Stunde lang und dieses in der Zeit sehr oft und fielmahl ich dachte es würde ein Nordscheins Bogen formiren aber es blieb nur bey den streif welcher oft sehr helle erschien, aber entlich nach oftern verschwinden nicht wieder kam, im übrigen war der Himel gantz klahr wir sahen auch den Mond aufgehen die Stadt uhren schlugen 4 Uhr.

While the above was in the press, Mr. Lohse has called my attention to the observations of the comet of 1743 by "Margaretha Kirchin" published in the Astr. Nachr. No. 44, so that there can be no doubt that this was the name of the observer in 1744. The diaries now at Dun Echt do not contain the observations published by Olbers, but the diary for 1743 has evidently been kept by a different member of the family, and it is frequently mentioned in it that "M. K." observed the comet.

RECENT LITERATURE.

Untersuchungen über die Präcessionsconstante auf Grund der Sterncataloge von Lalande und Schjellerup. Inaugural-Dissertation von F. Bolte. Bonn 1883 (28 pp. 8vo). In Vol XVII. pp. 255–259 of the Vierteljahrsschrift, Professor Schönfeld has at the end of a review of Dreyer's paper on the constant of Precession considered the mathematical relations between the Precession, the motion of the Solar System and a systematic rotation of the Fixed Stars supposed to take place in a direction parallel to the plane of the Milky Way. In the present paper the Author endeavours by using Professor Schönfeld's formulæ to determine the amount of the constant of Precession and to find whether this hypothetical rotation exists or not. He uses the *Declinations* of the stars common to Lalande and Schjellerup, having first reduced Lalande's Declinations anew by von Asten's tables. The comparison showed not only a number of deviations arising from Proper Motion, but in a number of cases a reference to other Star Catalogues showed that Lalande had erred either $10''\cdot0$ or $15''\cdot0$. A complete list is given of all these errors as also of the Proper Motions detected. The remainder of the investigation is carried on in three different ways, first making use of all stars which showed large differences (Schj.—Lal.) which could not be clearly explained, next excluding all these, and finally leaving out all stars possessing a Proper Motion of $0''.2$ or more in a great circle, while in the two first calculations only stars with a P.M. in Declination larger than $0''.33$ were excluded. The stars were furthermore sorted in three groups, the first containing stars fainter than 8·3 mag., the second stars from 7·5 to 8·2 mag., the third from 5·5 to 7·4 mag., all the magnitudes being taken from Argelander's and Schönfeld's Durchmusterungen. For every hour the mean value of $d\delta$ was taken for each group of magnitude, and 24 equations of condition were formed of the form $x \cos \alpha + y \sin \alpha + z = d\delta$, where x, y and z represent the coefficients in Schönfeld's formula for $d\delta$ (l.c. p. 256), the third and fourth term being = 0 on account of the stars being grouped round the Equator. The three groups did not show any systematic difference in the values of x, y, z, depending on magnitude. They all give the same sign for the change of galactocentric longitude (dl) of the star, but the numerical values of this change are so small that the agreement of the signs probably only arises from some constant error. The three calculations give corrections to the Lunisolar Precession ($d\psi$) and adopting Peters' values of the planetary Precession &c. the General Precession for the year 1800 is found equal to respectively $50''.2197$, $50''.2183$ and $50''.2234$. The Author next makes use of the 24 values of $\Delta\alpha$ given in *Copernicus* Vol. II. pp. 152-153, first having reduced them to Newcomb's system. They furnish 24 equations of condition $d\alpha = u + v \sin \alpha + w \cos \alpha$, where u, v and w likewise represent the coefficients in Schönfeld's formula for $d\alpha$. The combination of u with x and y gives however values of $d\psi$ equal to about $+ 0''.6$, while the Declinations alone gave about $- 0''.8$. This discrepancy may either arise from the general uncertainty of the problem or from the supposition that the plane of rotation of the Fixed Stars is parallel to that of the Milky Way, but in the latter case we have not data enough to separate the Precession from the Rotation as long as not either the node or the inclination of the plane of rotation is known through other means. Combining R.A. and Decl. the three calculations give for the General Precession for 1800 ; $50''.2407$, $50''.2417$ and $50''.2426$.

Dr. Bolte's paper is chiefly interesting as a numerical application of Schönfeld's formulæ, as the materials at present available are not extensive enough to enable us to prove clearly whether systematic motions in the plane of the Galaxy exist or not. As to the values produced of the Constant of Precession, it is significant that the Declinations when used by themselves give a value differing much more from the generally adopted value than the Right Ascensions do. Possibly this may to some extent be explained by errors im Piazzi's or Lalande's Declinations.

SUR LES CONFIGURATIONS DES COMÈTES.

PAR THÉODORE SCHWEDOFF.

Dans le No. 27 de ce Journal, M. Bredichin a bien voulu faire à mes travaux sur les queues cométaires quelques objections, que je ne crois pas possible de laisser sans réponse.

L'Astronome de Moscou trouve ridicule, que j'ose entamer une polémique avec lui. Voici mes droits à cette polémique. M. Bredichin croit avoir "établi ce fait capital de la constance de la force $1 - \mu$ pour chacun des types dans toutes les comètes". Voyons ce qui a lieu réellement.

D'après ses calculs, la valeur de $1 - \mu$ est contenue dans les limites 1·3 et 0·2 pour la queue principale, et dans les limites 0·3 et 0·2 pour la queue secondaire de la comète 1865 I ; elle varie entre 0·8 et 2·6 pour les comètes 1744 et 1858 V ; dans la comète 1874 seule, cette force serait égale tantôt à 2·4, tantôt à 0·2.[*] Or, comme il est impossible de considérer comme constante une valeur qui change dans le rapport de 1 à 10, le fait capital de l'Auteur nous paraît pour le moins douteux. Quant aux queues du premier type, la force $1 - \mu$ ne reste dans les limites $10 - 12$, qui lui sont assignées par l'Auteur, qu'à la condition d'écarter tous les cas, où ces limites sont sensiblement excédées. Par exemple, pour la comète 1862 II, l'Auteur trouve $1 - \mu = 80$,[†] et puisque cette valeur dépasse évidemment les limites ci-dessus indiquées, l'Auteur présume que la figure de la queue "a subi quelques perturbations." Mais le fait vraiment capital qui ressort des études de ce savant, c'est que la théorie des forces répulsives émanant du Soleil se trouve en désaccord manifeste avec les faits observés. En effet, la direction calculée des conoïdes qui constituaient la comète de 1744 dévie de 30° à 60° de leur direction observée. Ce désaccord n'echappe pas à l'Auteur, mais il tâche d'amortir le coup qu'en ressent sa théorie, en soupçonnant Chéseaux d'une erreur considérable. Une surprise analogue se retrouve dans la comète de Donati ; la direction des conoïdes observés dans la partie supérieure de celle-ci, après son passage au périhélie, dévie de 20° à 50° de leur direction théorique. Cette fois, M. Bredichin pare le coup en passant sous silence les dessins de Bond pour qui ces conoïdes étaient "distinctly outlined and separated by clearly marked dark intervals,"[‡] et invoque le dessin de M. Winnecke, pour lequel "leider war die Erscheinung so schwach, dass sie sich einigermassen exacten Beobachtungen ganz entzog."[§] Qu'on n'aille pas croire que le désaccord indiqué provient d'un choix erroné de la valeur numérique de la force $1 - \mu$; *aucune* valeur de cette force, fût-elle choisie entre les limites $-\infty$ et $+\infty$ ne conviendrait aux directions des conoïdes des deux comètes que je viens de nommer. Le fait est, que d'après la théorie des forces répulsives, représentée par M. Bredichin, la direction de ces conoïdes est toujours longitudinale par rapport à la direction générale de la queue, tandis qu'en réalité elle à été dans ces deux comètes transversale. Qu'on compare la queue-type de M. Bredichin (*Copernicus* Vol. I, Pl. 3) au dessin de Bond représentant la comète de Donati le 12 Octobre 1858.

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[*] Annales de l'Observatoire de Moscou, Vol. VI., Livr. 1 pp. 60, 48, 49, Vol. III. Livr. 2, p. 11, Copernicns I, p. 114.

[†] Annales III, 1, 28.

[‡] Bond, Account &c., p. 163.

[§] Mémoires de l'Académie de St. Pétersbourg T. II, No. 1, p. 66.

Tout cela n'empêche pourtant pas que l'Auteur ne déclare plus tard ceci (Annales, VIII, 1, p. 81) : "Nous remarquons dans la queue du II type de la comète 1881 IV comme dans celle de la comète 1881 III la propriété de se subdiviser en plusieurs conoïdes, qui s'est manifestée plus clairement dans la comète 1858 V et surtout dans la comète de 1744 ... Chaque conoïde a sa valeur $1 - \mu$".

Je passe aux objections à ma théorie.

M. Bredichin me fait un reproche de ce que je remplace une conception si simple (celle des forces répulsives) par une périphrase très-embrouillée (des ondes se propageant dans l'espace céleste). Examinons cette periphrase embrouillée et cette conception simple.

D'abord, pour expliquer les formes des queues cométaires, M. Bredichin admet non pas une seule force, comme c'est le cas pour l'attraction universelle, mais bien trois types de forces. Ensuite, chacun de ces types doit se subdiviser en plusieurs variétés. Enfin, quand ces types et variétés ne suffisent plus, il a recours à diverses conceptions, comme: émissions, impulsions, substances cométaires, substances planétaires, influences des particules d'un type sur celles d'un autre, quelques perturbations, suppositions additionelles etc.* Tel est le bataillon de conceptions simples auxquelles je n'en oppose qu'une seule : une queue cométaire et une onde engendrée par le noyau dans le milieu cosmique.† Et puisque la figure d'une onde est une conception purement géométrique, je la soumets à l'analyse mathématique qui me conduit à ce resultat.

La direction de l'axe de la queue principale, aussi bien que la direction des bandes lumineuses qui traversent celle-ci, sont déterminées *uniquement* par la figure de l'orbite décrite par le noyau de la comète. Aucune constante inconnue, aucun paramètre arbitraire n'entrent dans les formules de ma théorie qui déterminent la figure de la queue aussi bien que sa structure stratifiée.

J'ai appliqué ma théorie, entre autres, aux comètes 1744 et 1858 V, et chose étrange, les mêmes comètes qui se sont montrées si rebelles envers M. Bredichin, devinrent dociles à ma théorie, quoique, au dire de ce savant, je les défigurasse par mes ondes. Il se manifesta que les colonnes lumineuses de Pape, les bandes de stratification de Bond, la zone la plus lumineuse de la queue de la comète de Donati coincident avec les directions exigées par la théorie. Même, je n'ai pas eu besoin de soupçonner l'observation de Chéseaux, car ses conoïdes coincident aussi avec la direction de mes ondes. Je m'en rapporte à tous ceux qui ont sous la main mon ouvrage ("Théorie mathématique des formes cométaires, Première partie, Formes fondamentales," Odessa 1880), et pour ceux qui ne l'ont pas, je ne citerai qu'un exemple : pour la comète de Donati, le 9 Octobre 1858, l'axe calculé de la queue ne s'écarte de l'axe observé que de 40′ sur un parcours de 30°. Cela s'appelle "tirer par les cheveux une ressemblance superficielle."

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* Annales de l'Obs. de Moscou, III 1, 28, 44; IV 1, 77, 78, Copernicus II, 56.

† Idées nouvelles sur l'origine des formes cométaires, Odessa 1877 p. 8.

Passons aux prédictions.

Il y a un an et demi, quand la comète 1882 I était à peine visible, j'ai essayé d'en prédire la configuration à venir ("Sur les configurations de la grande comète 1882 *a*," Odessa 1882). Le dessin ajouté à ma Notice et destiné à illustrer les résultats des calculs, montre ce-ci : la queue principale de la comète est presque rectiligne avant le passage au périhélie et très-recourbée après ce passage, surtout dans les parties supérieures (éloignées du noyau). En outre cette queue est rayée de bandes transversales, lesquelles, étant très-serrées dans le voisinage du noyau, ne deviennent séparées, prononcées que dans les parties supérieures de la queue. J'ajoute : "si la queue n'est pas assez lumineuse, on n'en verra que les parties les plus rapprochées du noyau."—" Les astronomes ont vu comment les choses se sont passées" replique M. Bredichin.

Or, il s'est passé ceci. La comète, comme l'atteste cet astronome lui-même, "n'a pas manifesté la propriété de développer une queue considérable"; les astronomes n'en ont observé que la partie la plus rapprochée du noyau (relativement aux dimensions représentées sur mon dessin) et n'ont vu ni ses parties supérieures, ni, par conséquent, les bandes dont celles-ci ont été rayées. Une chose analogue a eu lieu pour la comète de Donati, dont les bandes transversales sont restées invisibles pour tous les astronomes du monde excepté pour Bond. Entre autres, ni Pape, ni Schmidt ni Winnecke ne laissent rien voir de ces bandes sur leur dessins des 12 et 13 Octobre, tandis que d'après Bond c'est le 12 Octobre que ces bandes ont été le plus prononcées.

Mais M. Bredichin considère l'absence des bandes transversales sur les dessins de la comète 1882 I comme un démenti à ma théorie, et même il pousse la négation au point de dire : "les queues stratifiées d'une telle manière n'ont jamais été observées." Mais quoi qu'il en soit de la partie invisible de la comète 1882 I, le fait principal qui doit nous intéresser pour le moment, c'est que la partie visible de cette comète, la seule sur laquelle on puisse discuter raisonnablement, s'accorde parfaitement avec la position qui lui est assignée par ma théorie. J'en prends à témoin M. Bredichin.

Car malgré la divergence de nos théories, il y a un cas où elles s'accordent toutes les deux avec l'observation, c'est quand la matière lumineuse des comètes se propage uniformément, en direction rectiligne tangente à l'orbite du noyau et avec la vitesse de celui-ci au moment du départ de ses particules. Suivant M. Bredichin c'est un cas particulier, exclusif, qui a lieu quand $1-\mu=1$, c'est à dire, quand la force répulsive est égale à la force attractive. Suivant ma théorie, c'est un cas général, ayant lieu pour toutes les queues principales (II type de M. Bredichin) et qui s'explique par la propriété des ondes cosmiques de se propager uniformément, en direction rectiligne, tangente à l'orbite du noyau et avec la vitesse de celui-ci au moment du départ de ces ondes (Théorie math. p. 15).

Il s'agit de reconnaître si l'égalité $1 - \mu = 1$ est vraiment un cas particulier.

Si l'on étudie les travaux de M. Bredichin, on ne tarde pas à s'apercevoir que les nombres trouvés par lui pour la force répulsive $1 - \mu$ du II type, sans être *constante*, comme il l'assure, sont néanmoins contenus dans des limites très voisines à l'unité. Pour la comète 1874 III ces nombres varient entre 2·4 et 0·2, et la moyenne en est de 0·8 ; cette moyenne est égale à 0·6 pour la comète 1860 III, à 0·7 pour 1865 I etc. Mais laissons parler l'auteur lui-même.

"Le second type contient des particules pour lesquelles $1 - \mu$ varie entre 0·8 et 2·6, la moyenne = 1·2" (*Copernicus* I, p. 100).

"J'ai montré que pour ces queues (II type) la force $1 - \mu$ ne diffère pas sensiblement de l'unité" (ibid. p. 114).

"La plupart des points observés (de la comète 1881 III) s'accordent avec la position de cet axe de la queue calculée" (en supposant $1 - \mu = 1$; ibid. II p. 9).

Dans la comète 1881 IV la direction de l'axe calculé dans l'hypothèse $1 - \mu = 1$ "est précisément la même dans laquelle M. Bruhns a cru voir la seconde queue" (ibid. II p. 18).

Enfin, dans la comète 1882 I "les variétés des répulsions sont comprises, après le périhélie, entre 0·8 et 1·6, dont la moyenne arithmétique $1 - \mu = 1·2$" (ibid. III p. 44).

Ainsi, à mesure que l'on avance dans ces recherches, la force $1 - \mu$ s'approche de plus en plus de l'unité, et le cas particulier menace de devenir singulièrement fréquent. Il en résulte pour la matière cométaire cette propriété, étonnante dans une matière, de se transporter dans l'espace d'un mouvement rectiligne et uniforme, en dépit des forces attractives et répulsives qui n'existeraient au monde que pour se détruire mutuellement. C'est cette "conception simple" qu'on oppose à l'hypothèse des ondes se propageant dans un milieu cosmique.

M. Bredichin s'inquiète de ce que la théorie des ondes cosmiques serait impuissante à expliquer les queues moins déviées du rayon vecteur, "dont nous avons un exemple dans la grande comète de 1744 avant le périhélie." Quant aux forces répulsives de cette comète, j'ai déja montré à quoi il faut nous en tenir, et quant à l'impuissance de la théorie des ondes cosmiques, elle est en bonne voie, et si elle progresse lentement, c'est que le chemin, que M. Bredichin croit avoir aplani et préparé, est bien rude à parcourir.

En voici encore un exemple. Il s'agit de la queue-enveloppe. Le problème des ondes, engendrées par un noyau dans un milieu dépourvu de l'élasticité, conduit à plusieurs solutions. Une de ces solutions est l'onde-enveloppe, qui avant le périhélie doit précéder le rayon vecteur. "Or," dit M. Bredichin, une queue pareille n'a jamais été vue, depuis que les astronomes observent les comètes." Mais alors, que doit on penser de ces paroles du même astronome, émises il y a un an et demi (*Copernicus* II p. 12) : "la queue serait donc en avant du prolongement du rayon vecteur" (observation de M. Markwich, comète 1881 III, 15 jours avant le périhélie). Et que signifie cette reserve : *serait* en avant ?

En voici l'explication. En vertu du théorème des aires, l'aire décrite par le rayon vecteur d'une particule émise par le noyau, doit être égale à l'aire décrite en même temps par le rayon vecteur du noyau même. Il en resulte qu'aucune particule, séparée du noyau par l'action d'une force quelconque émanant du Soleil, ne pourrait dévancer le prolongement du rayon vecteur du noyau. Donc, avouer franchement qu'une queue observée *est* en avant de ce rayon, c'est réduire l'hypothèse des forces répulsives ad absurdum. De là le conditionnel "serait."

Odessa, Mai 1883.

NOTE SUR LA QUEUE DU PREMIER TYPE DE LA COMÈTE 1882, II.

Par TH. BREDICHIN.

Le description de la grande Comète de 1882, faite par M. Cruls (Comptes Rendus, 6 Nov., 1882) pour le 25·83 Sept. t.m. de Greenwich et citée dans mon Mémoire sur cet astre, indique clairement dans la queue l'existence d'un appendice, qui par sa direction et plus encore par sa longueur ("la longueur totale de la queue à près de 30°") exigeait une très grande force répulsive du Soleil.

M. Cruls ne donne pas des coordonnées de la queue, et c'est par cette raison que dans mon Mémoire je n'ai pu traiter ce sujet que qualitativement, en calculant pour le 25·8 Sept. la longueur et la position relatives des queues du I et du II types.

Mais au mois de mai de 1883, M. Elkin, l'abile astronome de l'observatoire au Cap de Bonne-Espérance, m'a envoyé amicalement son esquisse de la comète pour le 22·6 Sept., t.m. Gr. (Pl. V, Fig. 1). Cette esquisse s'accorde parfaitement avec la description de M. Cruls ; on y voit de même cet appendice remarquable, et les coordonnées de son bout y peuvent être estimées avec un degré d'approximation assez suffisant. Le croquis de M. Elkin est accompagné des remarques suivantes—

"Mais je ne puis m'empecher de vous envoyer une petite esquisse de l'originale dans mon *Uranometria Nova* de l'aspect de la comète le 22 Sept. Le mince rayon parfaitement droit et élancé n'a pas été vu en Europe, à ce que je sache, et pourrait vous intéresser. L'intensité en est beaucoup exagerée dans le croquis en comparaison avec la brillante chevelure qui s'étendait jusqu'à 8° ou 9° de la tête. Ce rayon disparut quand la Lune s'approcha de la comète, vers la fin de Septembre et je ne l'ai pas retrouvé avec certitude depuis. J'ai marqué les seules étoiles qui étaient visibles et par rapport auxquelles j'ai estimé la *position du bout de ce rayon,*—l'incertitude peut bien en être 0°·5 ou plus."

On voit, que l'attention de M. Elkin a été portée principalement sur le bout de la queue longue ; la position de la queue claire, comme plus éloignée des étoiles de comparaison, reste beaucoup plus incertaine.

Prenons (du croquis original envoyé par M. Elkin) les coordonnées : du bout de la queue longue (b), du point où elle se détache de la queue claire (c) et du milieu de l'extrêmité de cette dernière queue (e).

Ces coordonnées a et d des points observés et leurs coordonnées α et δ réduites à l'équinoxe moyen de 1882·0 seront—

	a	d	α	δ
b	145° 18′	— 4° 45′	145° 48′	— 5° 1′
c	156 45	— 2 45	157 18	— 2 58
e	156 45	— 3 20	157 18	— 3 33

A l'aide des éléments de M. Chandler (Astr. Nachr., 2470), et avec A et D donnés dans mon Mémoire on trouve pour le temps M = 22·6 Sept., les coordonnées du noyau α_1, δ_1, v, r et p ; les angles de position p et $p°$ des points observés et du rayou vecteur prolongé et les autres quantités qui sont nécessaires pour les réductions au plan de l'orbite ; d'où on obtient finalement les distances des points observés angulaires (s) et linéaires (Δ) au noyau et les angles ϕ de ces Δ avec le rayon vecteur prolongé. Ainsi—

$\alpha_1 = 165° 45' 55''$ $\qquad$ $\lg r = 9{\cdot}5187464$ $\qquad$ $P = 164° 29'{\cdot}8$

$\delta_1 = -2\ 18\ 11$ $\qquad$ $\lg p = 0{\cdot}075009$ $\qquad$ $s = 97\ \ 15{\cdot}4$

$v = 162\ 19\ 40$ $\qquad$ $p° = 260° 52'{\cdot}3$ $\qquad$ $P' = 107\ \ 1{\cdot}2$

Points	p	s	T	ϕ	Δ	Δ en millim.
b	261° 43′·6	20° 11′	134° 40′	+ 3° 37′·9	0·9648	72·4
c	261 24·6	8 31	133 23	+ 2 20·1	0·2846	21·3
e	265 19·8	8 31	145 52	+ 15 4·3	0·4062	30·5

Le signe + devant ϕ signifie la position du point en arrière du rayon vecteur prolongé. Pour la construction graphique l'echelle est 1 = 75 millimètres.

Les points b, c, e sont portés sur la planche V (Fig. 2), et désignés par des petits cercles. La ligne $O\xi$ est le prolongement du rayon vecteur du noyau.

Calculons maintenant les forces nécessaires pour la formation des queues. L'axe optique, et par consequent le point e, de la queue claire correspond approximativement, comme on le voit dans mon Mémoire, à la force $1 - \mu = 1$, c'est a dire un mouvement uniforme sur la tangente à l'orbite avec la vitesse du noyau au moment de l'émission des particules de la queue.

Ainsi, calculons exactement la position de la tangente à l'orbite mené par le point e. Les coordonnées du point (t) de tangence seront—

$\lg r_1 = 8{\cdot}6565116$, $v_1 = 131° \; 1' \; 24''$ et le temps correspondant $M_1 = 17{\cdot}54367$ Sept.

Maintenant il est facile de calculer la longueur (l) de la partie de la tangente entre les points e et t, et l'on obtient $\log l = 9{\cdot}8378628$.

Pour obtenir la vitesse H des particules du bout de la queue claire il faut diviser cet l par la différence des temps $M - M_1$ (ou l'unité de temps est 58·13244 jours) et l'on a $H = 7{\cdot}92$, tandis que la vitesse du noyau pour le moment $M_1 = 17{\cdot}54367$ Sept. est $H = 6{\cdot}64$. Cette différence s'explique par l'incertitude de la position de la queue claire sur le croquis. Faisons en effet le calcul *inverse* de la position du point e pour le moment M = Sept. 22·6 avec la vitesse du noyau pour le moment $M_1 = 17{\cdot}5$ Sept. Ce calcul nous donnera $\phi = + 19° \; 38'{\cdot}0$ et $\Delta = 0{\cdot}3312$, ou 24·8 millim. Le point calculé est porté sur la planche V. (Fig. 2), et y désigné par une petite croix et l'inscription $1 - \mu = 1$.

Avec la valeur de ϕ on trouve l'angle de position p calculé pour le point e. Ce $p = 267° \; 23'{\cdot}9$, tandis que p observé est $265° \; 19'{\cdot}6$, la difference de 2° (correspondante à une erreur d'á peu près 1° dans la déclinaison du point e) est plus que modique pour la position de la queue claire sur le croquis. D'ailleurs cette queue claire n'est pas très intéressante pour le moment, car nous l'avons déjà étudiée beaucoup plus amplement dans son développement complet et d'après les observations exactes.

Passons donc à la queue longue, à ce phénomène très important pour la théorie et faisons premièrement la question : ne pourrait on pas expliquer sa formation par le mouvement de ses particules sur les tangentes à l'orbite, avec les vitesses respectives du noyau ?

Menons donc la tangente á l'orbite par le bout de la queue b.

Les coordonnées du point de tangence (t) cherché seront $\log r_1 = 8{\cdot}8516027$, $v_1 = 141° \; 19' \; 35''$ et le temps correspondant $M_1 = 17{\cdot}8173$ Sept. D'où l'on a pour la longueur de la partie de la tangente entre les points t et b $\log l = 0{\cdot}0889233$, et en divisant cet l par l'intervalle de temps $M - M_1$ on obtient $\log H = 1{\cdot}1736697$ et la vitesse *moyenne* $H = 14{\cdot}92$ (59 lieues géogr. par seconde), tandis que la vitesse du noyau pour le moment $M_1 = 17{\cdot}8173$ est $H = 5{\cdot}31$ (21 lieues par seconde) ; la première est 2·8 fois plus grande que la dernière ; c'est au périhélie seulement, ou $v = 0$, que la vitesse du noyau est égale à 16·02 (63·8 lieues géogr. par seconde).

Voila l'une des plusieurs raisons par lesquelles l'étude sérieuse des comètes a dû conduire à l'admission de la force centrale répulsive (réelle ou apparente), dont la cause physique nous reste inconnue comme celle de l'attraction Newtonienne. Ou peut être que les " ondes cosmiques " ont la propriété occulte de se propager avec la vitesse exigée spécialement par chaque forme cométaire observée et de se manifester toujours là, où on leur ordonne de devenir visibles ?

Vu la faiblesse de la queue longue, il est tout naturel de supposer, que sa longueur a dû être estimée au dessous de son étendue réelle ; mais admettons le cas contraire, moins probable, que sa longueur n'était pas égale à 20°, comme nous l'avous trouvé, mais à 19°. Dans ce cas ou trouvera $\log l = 0{\cdot}02026$ et $H = 12{\cdot}7$, qui est toujours 2·4 fois plus grande que la vitesse du noyau au moment M_1.

Calculons maintenant avec une valeur provisoire de la force du I type $1 - \mu = 12$ la position du point b. Pour l'intervalle de temps donné ci-dessus la formule de Bessel est suffisante, et nous aurons pour la valeur de $\Delta = 0{\cdot}9648$ l'angle $\phi = +\ 5^\circ\ 45'{\cdot}8$ (Fig. 2, petite croix et l'inscription $1 - \mu = 12$). Avec ce ϕ on trouve $p = 262^\circ\ 13'{\cdot}1$, et p observé est $261^\circ\ 43'{\cdot}6$. La différence de $30'$ correspond à l'erreur de $10'$ en dèclinaison, c'est à dire elle se trouve tout à fait dans les limites des erreurs admissibles d'après la remarque de M. Elkin.

Il est à remarquer ici que non seulement la direction de la queue, mais quelquefois aussi la lougueur peut servir à discerner les types, comme j'avais eu des occasions de le montrer dans mes recherches.

Sur la Fig. 2, la ligne *Om* donne la position de l'infortunée *queue-enveloppe* de la théorie des ondes cosmiques, de cette queue condamnée à l'invisibilité permanente dans tous les cas favorables à sa distinction.

Revenons pour un moment á l'estimation de M. Cruls, d'après laquelle la queue longue embrassait (le 25·83 Sept.), un arc de 27° à 30°. Pour cette queue la valeur de Δ est approximativement de 1·303 à 1·447 et par conséquent elle a reçu dans trois jours un accroissement de 7·5 à 10 millions de lieues géographiques. La partie de la tangente (calculeé plus haut pour la queue longue du 22 Sept.), comprise entre le point de tangence et le bout de la queue du 25 Sept., est de 1·686 à 1·831 ; avec l'intervalle de temps 25·83 — 17·82 on obtient la valeur de la vitesse moyenne H de 12·2 à 13·3 (l'unité de temps est 58·13244j), qui est de 2·3 à 2·5 fois plus grande que la vitesse du noyau au point de tangence 5·3. A cause de la faiblesse de la queue longue et de son alongement énorme une partie de son extrêmité (quelques degrés) vue le 22·6 Sept. a dû même devenir invisible le 25·8 Sept.

Je veux ajouter encore quelques mots par rapport à la masse cométoïdale observée par M. Schmidt (Astr. Nachr., 2468), le 9, 10 et 11 Oct. et vue par M. Barnard (ibid., 2489) le 14 Octobre.

J'ai taché de le mettre en connexion, en dependance, avec le noyau de la comète á l'aide des explosions de valeur admissible, ayant lieu près du périhélie actuel,—mais tout en vain.

En l'approchant du noyau, tant que les observations le comportent, et en la plaçant tantôt dans le plan de l'orbite et tantôt dans le plan perpendiculaire à la ligne visuelle,—on obtient pour elle les vitesses hypérboliques de 1·6 à 2 fois plus grandes que la vitesse du noyau, ce qui exige des suppositions très hardies par rapport à la force d'explosion du noyau. Toutes mes tentatives me reconduisent à

Comète 1882, II. Sept. 22.6 Greenwich.

Fig. 1.

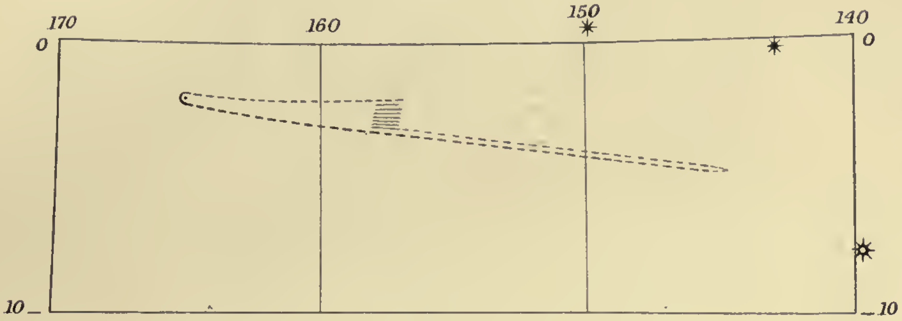
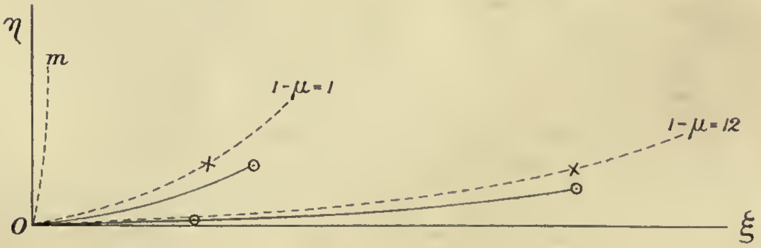


Fig. 2.



l'orbite parabolique calculée par M. Oppenheim (ibid., 2466), d'après laquelle la masse lors des observations se trouvait à 10 millions de lieues géogr. du noyau, et me font aussi croire que la connexion intime de cette masse avec le noyau de la grande comète est plus que douteuse. Ou peut être que la masse s'est détachée de la comète lors du périhélie antécédent, comme après le périhélie actuel le noyau s'est partagé en plusieurs parties, qui s'eloignent l'une de l'autre de plus en plus.

L'existence du tuyau large DD′ de M. Schmidt est confirmée par l'observation de M. Landreth (Astr. Nachr., Vol. 105, dessin).

1883, 25 Juillet.

QUELQUES MOTS SUR LES COMETES; RÉPONSE À M. SCHWEDOFF.

PAR TH. BREDICHIN.

Provoqué pendant quatre ans par la polémique de M. Schwedoff, j'avais publié enfin dans le No. 27 de ce Journal, à la fin de mon article sur la comète 1882 I, une Note sur la théorie des ondes cosmiques. M. Schwedoff a repondu à cette Note dans son article "Sur les configurations des Comètes," où il expose les motifs de sa polémique.

Commençons par ses citations.—"Le second type contient des particules pour lesquelles $1-\mu$ varie entre 0·8 et 2·6 (la moyenne 1·2)." [Je parle ici des comètes 1858 V et 1744, la moyenne est à M. Schwedoff, sa valeur exacte est 1·7.]

"J'ai montré que pour ces queues (II type) la force $1-\mu$ ne diffère pas sensiblement de l'unité." [La citation est inexacte, car je dis : "Les queues du second type (hydrocarbures) sont souvent aussi simples et j'ai montré que pour ces queues les forces $1-\mu$ ne diffère pas sensiblement de l'unité. Mais . . ."]

"La plupart des points observés de la comète 1881 III s'accordent avec la position de cet axe de la queue calculé (en supposant $1-\mu=1$)." [Je continue "Mais il y a des points . . . dont les particules ont été mues par des forces plus grandes."]

"Dans la comète 1881 IV la direction de l'axe calculé dans l'hypothèse $1-\mu=1$ est précisément la même dans laquelle M. Bruhns a cru voir la seconde queue."—"Dans la comète 1882 I les variétés des répulsions sont comprises, après le périhélie, entre 0·8 et 1·6, dont la moyenne est $1-\mu=1·2$."

A l'aide de mes publications M. Schwedoff pourrait bien faire une table des nombres du II type beaucoup plus étendue. Et voici l'objection principale de M. Schwedoff : "M. Bredichin croit avoir établi ce fait capital de la constance de la force $1-\mu$ pour chacun des types dans toutes les comètes . . . . Si l'on étudie les travaux de M. Bredichin, on ne tarde pas à s'apercevoir que les nombres trouvés par lui pour la force répulsive $1-\mu$ du II type, sans être constants, comme il l'assure, sont néanmoins contenus dans les limites très voisines à l'unité." Est ce que ces phrases contradictoires de M. Schwedoff font une réfutation de mes déductions? Les nombres cités montrent seulement dans quel sens j'entend la constance des forces dans mes types. M. Schwedoff, qui n'a fait qu'une application bien incomplète de ses raisonnements à un petit nombre des comètes, tache de puiser dans mes travaux la persuasion que mes nombres sont contenus dans des limites très voisines à l'unité ; quant à moi, je trouve depuis longtemps que ces nombres obligent de rejetter cette particule : *très.* Si les forces ne fussent que trois, je n'aurais pas dit : *trois types*, mais tout simplement : *trois forces.* Je trouve ce que me donnent les observations et les calculs, et j'assimile avec réserve les variétés des nombres aux variétés des substances en général et des hydrocarbures dans le II type.

Pour M. Schwedoff c'est autre chose. Il lui faut à tout prix ce $\mu = 0$, car ce genre de mouvement (rectiligne et sans force) est le plus apte aux périphrases de toute sorte. Une périphrase pour le I type est déjà compliquée et M. Schwedoff n'y a pas reussi avec sa queue-enveloppe (voire ma Note). Il adopte donc que notre cas particulier ($1 - \mu = 1$) est le seul possible dans le II type, il en fait la périphrase à l'aide de sa conception intuitive des ondes et ce cas devient pour lui " un cas général, ayant lieu pour toutes les queues principales (II type de M. Bredichin) et qui s'explique par la propriété des ondes cosmiques de se propager uniformement, en direction rectiligne, tangente à l'orbite du noyau et avec la vitesse de celui-ci au moment du depart de ces ondes."

Notons bien ce *principe* (à vitesses et directions *déterminées*) qu'il simplifie ainsi (pour un moment, pour vanter la simplicitié de sa théorie) pour le II type. A la fin de la réponse de M. Schwedoff on trouve encore que "les *ondes* sont engendrées par un noyau dans un milieu depourvu de l'élasticité." (?) Un milieu a remplacé l'espace rempli des corpuscules se mouvant suivant les lois de Kepler. Dans l'exposé de la théorie le noyau engendre en général à chaque instant une multitude d'ondes de toutes les vitesses possibles.

D'après son principe M. Schwedoff est obligé de chercher partout notre cas $1 - \mu = 1$, où pour lui les ondes se renforcent mutuellement et deviennent ainsi les plus claires ; la bande des ondes renforcées est l'axe de la queue. Or, notre cas $\mu = 0$ étant identique (pour le calcul) avec le principe de M. Schwedoff, il s'en suit que quand mes calculs me fournissent outre $\mu = 0$ d'autres valeurs pour μ, ce principe devient défectueux. Si, par exemple, $1 - \mu$ est entre 0·8 et 2·6, la queue de M. Schwedoff se trouve près du bord postérieur de la queue observée. Comment remédie-t-il à ce défaut et de quoi remplit-il la largeur de la queue? Il y place quantum satis, en avant et en arrière de son axe, des ondes non renforcées, des ondes simples, qu'il trace perpendiculairement aux tangentes respectives. Dans le cas actuel ($1 - \mu$ entre 0·8 et 2·6) les séries des ses ondes faibles vont former les parties antérieures de la queue, plus claires que la bande des ondes renforcées.

Dans la comète 1882 II M. Schwedoff trouve son axe tout près du bord antérieur (Comptes Rendus, 7 Mai, 1883, p. 1349, figure), au dessus de cet axe on voit une bande obscure longitudinale (toujous niée par M. Schwedoff) parcourant la queue et puis vient toute une grande queue avec ses détails remarquables. D'après M. Schwedoff c'est un amas des ondes faibles, dont la figure échappe au calcul et reste arbitraire. C'est probablement l'écume des ondes cosmiques. Qu'on compare cette figure avec la forme prédite par M. Schwedoff pour la comète 1882 I: les conditions sont égales.

Dans la comète 1874 la bande des ondes renforcées tombe sur la bande obscure du conoïde. Quand le second type est absent, M. Schwedoff doit supprimer ses ondes renforcées et dans le III type on ne verra que les ondes faibles, etc.

Mais que sont enfin ces ondes renforcées ou axiales et ces ondes secondaires, faibles? Voyons.

Dans un instant donné le noyau engendre dans le milieu non élastique (antiéther ?) une onde d'une vitesse determinée *H* (d'après le *principe*) qui sera l'onde *axiale;* pour le moment de l'observation M. Schwedoff trace déjà sur la tangente réspective, en *avant* et en *arrière* de cette onde une *série des ondes de la même vitesse H* (?). La première onde se trouvera sur l'axe de la queue et les autres iront former le corps et les strata de cet appendice.

Les ondulations observées *à la surface* d'un liquide, M. Schwedoff les transporte, mal interprétées, dans *l'intérieur d'un milieu non élastique.* Il compare la queue au sillon que laisse derrière lui le navire, puis il érige en thèorie cette ressemblance superficielle et il tache ainsi de donner une explication physique à sa *périphrase* de ce cas particulier $1 - \mu = 1$. L'élasticité du milieu lui ferait plus des difficultés.

D'après sa conception non simplifieé, le noyau engendre à chaque instant un chaos affreux des ondes, mais il n'en repêche pour le II type que les ondes axiales (correspondantes au principe) et il supprime toutes les autres, en épargnant pourtant celles, qui, quoique non renforcées, lui sont nécessaires pour remplir la queue. Ces ondes secondaires, il ne les lie pas, il les laisse indépendantes, les separe par des intervalles observables et, en oubliant son principe et son axe qu'il interrompe, il les trace de manière à produire ces strata, qu'il veut retrouver dans quelques croquis vagues, isolés et incertains. Les comètes "devinrent dociles à ma théorie." Pour se tirer d'embarass et débrouiller ses conceptions *simples* il doit recourir à la pluie et à l'arc-en-ciel.

On peut demander encore: pourquoi les planètes et les astéroides (voir les distances des comètes 1825 IV et 1882 II), que M. Schwedoff ne veut pas distinguer des comètes dans ses "Illusions astronomiques" n'engendrent pas des ondes cosmiques, dans un milieu si favorable aux *ondulations*? M. Schwedoff a taché jadis d'éclaircir ce point, mais sans succès (voir sa "Théorie" . . .) et dupuis lors ces idées sur le milieu cosmique sont devenues plus déterminées et ce milieu, à ce qu'il parait, n'est plus pour lui un amas des corpuscles se mouvant suivant les lois de Kepler.

La simplicité mécanique de l'hypothèse des forces répulsives centrales (réelles et apparentes), dont la cause physique nous reste cachée comme celle de l'attraction, est connue de tout le monde, et je n'ai pas besoin de la défendre; elle est pour ainsi dire la simple description des phénomènes observés : la matière cométaire s'élance vers le Soleil et puis va à l'opposite de cet astre. M. Schwedoff dit, que j'emploie les mots : émission, impulsions, etc. Oui, et j'emploie aussi les mots : effluve, matiére caudale et quelques autres encore, mais les astronomes et les mécaniciens, qui liront mes travaux, sauront apprecier la portée de ces mots et mon droit de les employer.

Voici encore une grave objection par M. Schwedoff. " Dans la comète 1874, seule, cette force $(1 - \mu)$ serait égale tantôt à 2·4 tantot à 0·2. Or, comme il est impossible de considérer comme constante une valeur qui change dans le rapport de 1 à 10, le fait capital de l'Auteur nous parait pour le moins douteux." C'est une étrange méprise que M. Schwedoff reproduit plusieurs fois pendant quatre ans et toujours par rapport à la même comète. Il cherche partout son $\mu = 0$, et chez moi dans une méme queue peuvent exister plusieurs conoïdes et plusieurs μ. Mais je veux lui aider dans son objection. Le conoïde de la comète 1874 était simple, presque avec un seul μ. Or, pour ce conoïde nous avons plusieurs observations, les unes pour les points voisins du noyau, les autres pour les points vers l'extrêmité de la queue. Dans le calcul de la force les premières doivent donner des résultats beaucoup plus discordants que les dernières, et cette discordance aura lieu dans le calcul direct des éléments numériques de chaque théorie. Si M. Schwedoff aurait eu calculé d'après ces observations les vitesses de ces ondes (réduites à un moment donné) il aurait trouvé les mêmes discordances. Les observations près de la tête (comète 1874) sont pourtant assez nombreuses et la moyenne de leur résultats discordants est par cela en accord avec le résultat $1 - \mu = 1·2$ fourni (à l'aide de la formule exacte) par les points vers le bout de la queue. J'ai répété maintesfois que les points près de la tête ne sont pas favorables pour le calcul direct, c'est à dire pour l'évaluation de la force, et on peut les negliger dans ce cas en présence des autres observations ; mais ils peuvent être employées dans le calcul inverse, que je prefère depuis longtemps, calcul par épreuves.

Une méprise pareille est contenue dans la remarque que fait M. Schwedoff par rapport aux nombres du I type, où les unités équivalent, pour le calcul, au dixièmes du II type. " Quant aux queues du I type, la force $1 - \mu$ ne reste dans les limites 10–12, que . . . , p. ex. pour la comète 1862 II l'auteur trouve $1 - \mu = 80$." Cette citation est d'abord une anachronisme. La discussion de la comète dans la livraison citée n'est que préliminaire, comme je la nomme expressément. Elle est refaite un an plus tard (Annales V, 2, pp. 46–50) et d'après un procédé plus rationel. Puis le sens de mes paroles est un peu altéré dans cette citation (Annales III, 1, p. 28).

Dans l'opinion de M. Schwedoff, la théorie des forces répulsives se trouve en désaccord manifeste avec les faits observés dans les comètes de 1744 et 1858. Remarquons d'abord, qu' en général une théorie développée, qui a reussi à représenter l'ensemble des phénomènes principaux et des particularités dans toutes les comètes observées jusqu' à présent, ne peut point être ébranlée par quelques observations isolées et douteuses. se rapportant aux détails à peine visibles et discordants avec les fait analogiques constatés pour plusieurs comètes ou par plusieurs observateurs.

Les esquisses du *bout* de la queue de la comète 1744, qui ont été faites par Chéseaux, sont discordantes avec les observations de De L'Isle et incertaines sous quelques rapports, et j'ai énoncé la dessus mon opinion (Annales, VII, 1, p. 53). La découverte récente des observations faites à Berlin et réproduites dans ce Journal par M. Dreyer m'affermit complètement dans cette opinion. Pour éviter la prolixité je renvoie le lecteur à l'article de M. Dreyer, où l'on trouve aussi (pp. 105-107) un résumé de mon avis.

Quant à la comète Donati, M. Schwedoff dit avec triomphe, que "la direction des conoïdes dans la partie supérieure de celle-ci dévie de 20° à 50° de leur direction théorique," et puis: "Qu'on compare la queue de M. Bredichin (*Copernicus* I, Pl. 3) au dessin de Bond." Or, comment M. Schwedoff obtient il ces déviations? Il compare ma figure citée, qui est *calculée* pour $q = 0{\cdot}25$, avec la queue de la comète Donati, ou $q = 0{\cdot}6$, et la valeur de q a une influence très grande sur la courbure de la queue. Le lecteur ne se laissera pas induire en erreur et il saura apprecier ce genre de polémique. Dans ma collection cométographique je crois avoir tous les dessins et les déscriptions publiés de cette comète, y compris ceux de Bond. Je possède en outre quelques dizaines des croquis pour le mois d'octobre, aimablement envoyés par mes collègues. Enfin, nous avons observé la comète à Moscou dans des conditions très favorables. Or, avant de me servir des dessins je les ai tous convenablement controlés et confrontés, car on sait que les erreurs personelles et d'autre genre (voir le Mémoire du Professor Newton sur cette comète) y jouent un grand rôle et que la coordination des formes se trouvent hors de la queue. C'est de cette manière que j'ai reconnu que la vraie conception, la conception moyenne pour ainsi dire de la structure de la queue est représentée par ma queue-type. Les formes moins stables et à peine observables, comme quelques nuages, quelques bandes, quelques noeuds, sont parfaitement explicables par l'intermittence et le déplacement du secteur d'émission (p. ex. dans la comète 1882 II les nuages de M. Schmidt et une corne au bout de la queue). Mes observations assidues ultérieures et mes recherches sur plusieurs comètes m'ont affermi dans mes idees de cette queue-type.

Quant aux strata et aux bords en lambeaux de la comète 1882 I, M. Schwedoff dit, qu'on ne les a pas vu à cause de son étendue trop petite (10°); en effet, on y a vu des formations incompatibles avec la théorie des ondes. Est ce que la queue de la comète 1882 II était aussi trop courte pour être comparée avec la figure prédite pour la comète 1882 I? La distance q était petite pour l'une et pour l'autre comète.

Pour chaque théorie on peut trouver quelques pierres de touche, quelques critériums. Pour la théorie des ondes, outre ce que nous avons dit plus haut, il y a plusieurs critériums: la structure conoïdale de la queue, la précission relative du bord antérieur, la formation de la queue à l'aide des émissions dirigées vers le Soleil produisant le creux des conoïdes même dans les comètes qui s'éloignent du Soleil presque en ligne droite (1882, II), la formation des queues dirigées vers le Soleil, etc., etc.

M. Schwedoff passe sous silence ou effleure à peine toutes ces choses importunes ; il renie des faits principaux et rehausse des traits insignifians, et il croit établir sa théorie "naissante" plutôt par la polémique contre les cométologues vivants et morts que par une comparaison plus ample et plus détaillée de cette théorie avec les matériaux amassés par l'observation.

Enfin, il y a encore un critérium, que M. Schwedoff a essayé, mais sans succès. Je parle des apparitions de la queue du I type, qui par son éclat devient quelquefois la queue principale et pour laquelle j'adopte jusqu' ici provisoirement (vu le manque d'observations) $1 - \mu = 12$. Elle a été visible dans plusieurs comètes, et chaque fois qu'on l'a vue elle était intimement liée avec le rayon vecteur prolongé, toujours en arrière et peu déviée de celui-ci. C'est un fait *très important*, et une théorie plausible doit le réproduir dans ces cas caractéristiques. Or, M. Schwedoff tache à tout prix de lier cet appendice avec la développante de la parabole, qui se trouve tantôt en arriére et tantôt en avant du rayon vecteur prolongé et leur angle depasse quelquefois 80°. En voici quelques cas caractéristiques de la position des queues I et II et de la développante D par rapport au rayon vecteur prolongé ; + signifie la position en arriére du rayon vecteur.

Comète 1807, Oct. 22, pour la queue I + 8°, pour II + 24°, pour D + 34°.

Comète 1744, pour I + 5°, pour II + 15°, pour D − 60°. La queue I longue de 25°, plusieurs fois observée.

Comète 1843 I ; pour I + 4°, pour II + 20°, pour D + 85°.

Comète 1769, une grande queue I, longue de 70° ; pour lui + 4°, pour D + 67°. Dans la comète 1882 pour I on a près + 4° (Elkin) et pour D + 80°. Voir encore les comètes 1680, 1811, 1865 et autres, qu'on trouvera dans mes écrits.

Dans ma Note j'ai posé clairement la question de la queue du I type, mais M. Schwedoff ne le résout que par la phrase suivante : "Il s'agit de la queue enveloppe. Le probléme des ondes, engendrées par un noyau dans un milieu dépourvu de l'élasticité conduit à plusieurs solutions. Une de ces solutions est l'onde-enveloppe, qui avant le périhélie doit précéder le rayon vecteur. Or, dit M. Bredichin, une queue pareille n'a jamais été vue, depuis que les astronomes observent les comètes. Mais alors que doit on penser de ces paroles du même astronome, émises il y a un an et demi (observation de Markwick, comète 1881 III, quinze jours avant le périhélie) : 'la queue *serait* donc en avant du prolongement du rayon vecteur.'"

Je remarque premièrement que M. Schwedoff invoque ici son principe non simplifié : le noyau engendre à chaque instant une multitude d'ondes des différentes vitesses. Dans la queue enveloppe, sur une même tangente à l'orbite, à chaque instant on voit des ondes nouvelles et d'une autre vitesse. Ce changement kaleidoscopique des ondes et des vitesses est égal à la variation de la vitesse parabolique du noyau. On nous menace des autres vitesses encore, plus compliquées : "plusieurs solutions." Oui, le principe est simple !

Voyons maintenant quelle est cette observation décisive de Markwick. On pourrait croire d'après la citation, qu'il y a du moins une observation donnant *indubitablement* la position de la queue en avant du rayon vecteur. Voici l'observation : "The direction of the tail was from this point (position du noyau) towards ζ Pictoris, the direction having apparently changed since last night." L'étoile de comparaison était éloignée *de* 12° *du bout de la queue;* ce n'est donc plus une observation tant soit peu exacte, c'est une indication approximative de la direction, qui donne pour la queue un angle de − 3°·6 (en avant du r. v.). Le lecteur pourra voir (ce journal, II, p. 12) avec quelle réserve j'ai taché d'expliquer cette observation, discordante d'ailleurs avec les autres, par la présence, avant le périhélie, de la queue du premier type, qui a été bien observée après le périhélie.

Et par une pareille donnée, incertaine et discordante, M. Schwedoff croit il réellement avoir prouvé le fait important et ignoré de l'existence des queues en avant du rayon vecteur prolongé et trouvé ainsi un soutien solide à sa théorie ? Voila ce que j'appelle : "tirer par les cheveux."

Th. Bredichin.

Kineschma, sur le Volga,
le 29 Juillet, 1883.

RECENT LITERATURE.

Resultate aus den in Pulkowa angestellten Vergleichungen von Procyon mit benachbarten Sternen. Von Ludwig Struve, St. Petersburg 1883, 48 pp. 4to (Mém. de l'Acad. Imp. des sciences, VII Série). In 1873 Auwers showed that all the measures of Procyon from neighbouring telescopic stars made up to that date agreed well with his circular elements deduced from meridian observations. Since then Procyon has been regularly observed in Pulkowa, and M. L. Struve has now utilized the observations for a new determination of the parallax and orbit. In 1852 M. Wagner commenced with the large Transit Instrument to observe the differences of R.A. between Procyon and four stars fairly symmetrically situated—

d	8·7 mag.	$P - d = +1^{m}\ 54^{s}\cdot 8$	$+3'\ 26''$
b	9 „	$P - b = +0\ \ 24\cdot 5$	$-1\ \ 30$
c	9 „	$P - c = -0\ \ 22\cdot 2$	$-0\ \ 44$
a	7·2 & 7·5	$P - a = -0\ \ 42\cdot 6$	$+1\ \ 45.$

The star *a* is the Double star Σ 1126, the centre being observed (the distance is 1″·3.) The observations were reduced to 1867·0, the mean Proper Motion of Procyon being taken from Auwers' Fundamental Catalogue. The results were—

From $P - d$	$\pi = +0''\cdot 390 \pm 0''\cdot 055$	$\delta a = -1''\cdot 020 \pm 0''\cdot 140$
„ $P - b$	$+0\cdot 327 \pm 0\cdot 073$	$-1\cdot 101 \pm 0\cdot 250$
„ $P - c$	$+0\cdot 307 \pm 0\cdot 071$	$-1\cdot 007 \pm 0\cdot 489$
„ $P - a$	$+0\cdot 383 \pm 0\cdot 055$	$-0\cdot 444 \pm 0\cdot 122$

where δa is the correction to the adopted semidiameter of the orbit, 0″·9805 These results were found by comparing each observation of Procyon with the

single observations of all the comparison stars taken on the same day, so that the four results are not independent of each other. The Author next treats the observations from 1863–68, which had been specially intended for a determination of the parallax, by taking a mean of the comparison stars observed on one day and forming the $\Delta\alpha$ between this mean and Procyon. In this way the relative parallax of Procyon was found $= +\ 0''\cdot299 \pm 0''\cdot038$. The star a is Bradley 1107 and has been very frequently observed on the meridian. From all available observations the Proper Motion was found $= -\ 0^{s}\cdot00255 \pm 0^{s}\ 00030$, and when this was inserted in the equations for π and $\delta\alpha$ from $P - a$, π was found $= +\ 0''\cdot398 \pm 0''\cdot061$ and $\delta\alpha = +\ 0''\cdot050 + 0^{s}\cdot100$, which results agree much better with Auwers'.

Since 1851 Otto Struve has observed the differences of R.A. between Procyon and the stars b and c, but the observations turn out to be far less accurate than might be expected, so that the instrument cannot have been firmly fixed in R.A. The observations of the difference of Declination from the same stars were specially intended to determine the irregularity of the Proper Motion. Those from 1851–73 were compared by Auwers with his circular elements and found to be very much better represented under the supposition of irregular than of uniform P.M. The Author shows that the entire series (1851–82) neither agrees with a uniform P.M. nor with Auwers' orbit. The latter certainly gives smaller residuals, but they clearly indicate corrections to the elements. As Procyon has only described about three-fourths of a revolution since 1851, Auwers' period had to be adopted. The following orbit represents the observations well—

$$\begin{aligned} a &= +\ 0''\cdot6980 \pm 0''\cdot0354 \\ T &= 1794\cdot966 \pm 0^{j}\cdot440 \\ n &= 9^{\circ}\cdot02993 \pm 0^{\circ}\cdot08072 \text{ (Auwers).} \end{aligned}$$

The epoch agrees well with that found by Auwers, but the semidiameter is much smaller than Auwers' value, the difference being seven times greater than the probable error of either result. One of the two series of observations employed must therefore be affected by systematic errors, and it is evident that this is far more likely to be the case with meridian observations than with the micrometer measures. The value of the semiaxis in the direction of the declination circle, found by M. Struve, satisfies all the observations used by Auwers within the limits of errors of observation, while on the other hand the Pulkowa micrometer measures clearly indicate a correction to Auwers' value. As M. Wagner's observations do not prove the necessity of altering the adopted value of the semiaxis in the direction of the parallel, we are led to the conclusion that the apparent orbit of Procyon is an Ellipse, the semiaxis major of which, parallel to the Equator, is $0''\cdot979$, while the semiaxis minor is $0''\cdot698$. The real orbit must therefore be a circle, perpendicular to the Declination plane and forming an angle of $45\frac{1}{2}^{\circ}$ with the line of sight.

OBSERVATION OF THE TRANSIT OF VENUS, 1882, DECEMBER 6, AT DUN ECHT OBSERVATORY.

By J. G. LOHSE.

(Communicated by the Earl of Crawford and Balcarres.)

On the sixth of December, 1882, the aspect of the weather was not very favourable. It was snowing repeatedly during the morning and even at times during the Transit, and the eastern sky remained mostly cloudy and threatening. At one o'clock it became clear in the south-west; thin clouds however passed occasionally over the Sun, and especially a small snow cloud, which obscured the Sun completely for about two minutes just at the time of internal contact, interfered seriously with the observation of that important moment. Otherwise the weather was best during the time of ingress and shortly after. At three o'clock a heavy snow shower came on and the Sun remained invisible for the rest of the day.

The observations were made with the 15·06-inch refractor and a power of about 190. The images were very poor the whole time, the limbs being distorted by violent boiling undulations. The image of Venus was sometimes transformed into quite irregular shapes, which cannot surprise as the light of the Sun was passing through the agitated air over the not very distant Hill of Fare. The limbs of Venus were very conspicuously coloured through atmospheric refraction, the upper limb being dark-blue and the lower red-brown. The intensity of the colours was rather remarkable, but it must be borne in mind, that the Sun was only a little more than five degrees above the horizon and that sometimes under similar conditions bright stars are seen as very short spectra, the only distinguishable colours of which, red and blue, are very bright.

Dun Echt Mean Time.	
$1^h\ 52^m\ 49^s$	First external contact. The limb of the Sun was boiling tremendously. The notch in the Sun's limb being only small, the recorded time cannot be very many seconds late.
2 5 54	The upper third of the visible circumference of Venus appears dark-blue and the lower two thirds red-brown.
2 8 19	Noticed a bright fringe round the part of Venus, still outside the Sun's disk; it remained visible until very near the time of internal contact, when no more attention was paid to it.
2 10 18	First internal contact. At this moment Venus appeared perfectly round and tangent to the Sun's limb, but in the latter was still a break, which seemed sometimes broad and sometimes narrow, the boiling undulations of the limbs being so very great. Immediately afterwards a small snow cloud rendered it impossible to see the first sun-light all round Venus, the time given must therefore be looked on as too early.
2 12 39	Saw through clouds the Sunlight all round Venus. The line of light between the two limbs was very narrow, indicating that the internal contact could not have occurred long ago.

It may therefore be taken for certain, that the real internal contact took place while the Sun was hidden by the cloud. In fact there could be little doubt that the internal contact had not yet occurred when the Sun became obscured, as the space between the cusps was too broad and dark; but the distinctness with which it could be seen, underwent great changes owing to the poor definition, so that for a few moments Venus appeared round and tangent to the Sun's limb.

When Venus had fully entered the Sun's disk, the prismatic colours on the limb were very plain; the upper or southern limb appeared dark blue and the lower one dull red.

The time was determined on the evenings of December 5 and 6, and also by α Lyræ shortly before the Transit.

SPECTROSCOPIC OBSERVATIONS OF COMET 1883, I. (made at Dun Echt Observatory with the Grubb spectroscope).

By J. G. LOHSE.

(Communicated by the Earl of Crawford and Balcarres.)

The spectrum of this not very bright comet could only be examined once, on March 1, 1883. For the observation I used the compound prism and the lowest Grubb eye-piece. The telescope was the 15·06-in. refractor.

The measurements give the following wave-lengths for the different parts—

		Wave-lengths. mmm.
Band 1.	Beginning of band 1,	568·2
	First maximum, extended and soft,	555·9
	A more defined bright part,	549·9
	End of band 1,	536·7
Band 2.	Pretty sharp edge of band 2. Brightest point,	517·0 516·8 516·4 516·1
	Another bright point, a little fainter than the first,	511·9 511·1
	The band is still bright at,	504·0
	End of band 2,	494·9
Band 3.	Brightest part of band 3,	472·8 471·9
	End of band 3,	460·7

Estimated relative brightness of bands.

Band 1 = 4
Band 2 = 12
Band 3 = 5

There is just sufficient continuous spectrum to connect the bands by a faint line; it is faintest between the first and second band.

OBSERVATIONS OF BROOKS' COMET (COMET OF 1812).

1. By Prof. P. Tacchini, R. Osservatorio del Collegio Romano, Roma.

1883.	t.m. Roma.	α app.		δ app.	
Sept. 8,	$9^{h}\ 45^{m}\ 57^{s}$	$16^{h}\ 30^{m}\ 45^{s}{\cdot}13$	(9·959)	+ 63° 50′ 44″·2	(9·335)
„ 10,	11 28 31	16 29 12·18	(9·989)	+ 63 24 39·0	(0·488)

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2. By Prof. H. G. v. d. S. Bakhuyzen, at Leiden Observatory.

"I observed Comet Brooks on the 5th and 8th September. The observation of 5 Sept. is not very good; I estimate the mean error in $\alpha \pm 0^{s}{\cdot}3$ in $\delta \pm 2''{\cdot}3$. The observation of Sept. 8 is a good one. The apparent places are—

	Mean time Leiden.	α app.	δ app.
5 Sept.	$14^{h}\ 42^{m}\ 0^{s}$	$16^{h}\ 33^{m}\ 16^{s}{\cdot}61$	+ 64° 25′ 38″·4
8 „	11 14 27	30 41·82	63 49 56·0"

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3. At Lord Crawford's Observatory, Dun Echt, with the 15-inch Refractor, by R. Copeland and J. G. Lohse.

Date. 1883.	Dun Echt Mean Time.	Comet − *		App. α Comet.	App. δ Comet.	Observer.
		$\Delta\alpha$	$\Delta\delta$			
Sept. 6,	$8^{h}\ 57^{m}\ 57^{s}$	$-1^{m}\ 9^{s}{\cdot}88$	+ 5′ 19″·2	$16^{h}\ 32^{m}\ 30^{s}{\cdot}89$	+ 64° 16′ 2″·5	R. C.
„ 8,	8 58 46	+ 1 23·88	+ 1 27·7	16 30 45·48	+ 63 50 58·5	„
„ 10,	13 9 0	− 6 36·10	+ 4 7·8	16 29 8·12	+ 63 22 57·5	J. G. L.
„ 11,	14 6 36	− 7 16·49	− 9 19·6	16 28 27·58	+ 63 9 30·0	„
„ 21,	9 20 42	− 1 10·00	+ 4 4·0	16 25 18·84	+ 61 0 35·3	„

Adopted mean places of Comparison Stars for 1883·0.

	α	Redn.	δ	Redn.	Authority.
Sept. 6,	$16^{h}\ 33^{m}\ 40^{s}{\cdot}89$	$-0^{s}{\cdot}12$	+ 64° 10′ 22″·9	+ 20″·4	A. Ö. 16400.
„ 8,	16 29 21·83	− 0·23	+ 63 49 10·9	+ 19·9	A. Ö. 16323.
„ 10,	16 35 44·41	− 0·19	+ 63 18 29·3	+ 20·4	Greenwich Obs. 1881, No. 697.
„ 11,	16 35 44·41	− 0·24	+ 63 18 29·3	+ 20·3	„ „
„ 21,	16 26 29·35	− 0·51	+ 60 56 12·9	+ 18·4	Rümker 5444.

Remarks:—Sept. 8. The comet is much brighter in the middle to a nucleus = 12 mag. star. The comet was about 20″ south a little preceding of a 12 mag. star.

Sept. 21. The comparison star (D.M. + 61° 1595) is double; the components are nearly of the same magnitude. From a single measure the position was found to be 73° and the distance 1″·5.

REMARQUES CONCERNANT LE PROBLÈME DE KEPLER.

Par M. R. RADAU.

Permi les nombreuses méthodes qui ont été proposées pour résoudre l'équation $E = M + e \sin E$, la plus simple est toujours la méthode indirecte de Gauss, qui part d'une première valeur E_0 ($M < E_0 < M + e$), après quoi $E_1 = M + e \sin E_0$, etc. Elle conduit rapidement au but, pourvu que E_0 soit déja une valeur suffisamment approchée de E. Cette première approximation est facilitée par les tables de M. Doberck (Astr. Nachr. 2202), qui donnent E pour tous les degrés de M et pour les dixièmes de e. Mais l'on peut arriver au même résultat à l'aide d'un diagramme formé seulement d'un système de liques droites, qui donne à vue la valeur cherchée de l'anomalie E.

En effet, si nous prenons M pour abscisse, e pour ordonnée, l'équation $M = E - e \sin E$ représente une droite le long de laquelle $E = \text{const.}$ Pour la tracer, il suffit d'observer que $M = E$ pour $e = 0$ et $M = E - \sin E$ pour $e = 1$. Les diagonales qui répondent à $E = 1°, 2°, 3°, \ldots$ passent par les divisions 1, 2, 3, . . . de l'axe des abscisses, et par les points $1° - \sin 1°$, $2° - \sin 2°$, ... de la parallèle $e = 1$. On pourrait encore les déterminer en marquant sur d'autres parallèles les valeurs de M pour les valeurs données de e, d'après la table de M. Doberck. Ces diagonales une fois tracées et numérotées, pour avoir E lorsqu'on connait M, e, on n'aura qu'à chercher le point de rencontre de la parallèle e avec la verticale M; la situation de ce point entre deux diagonales voisines E', E'', fera connaître E. Pour augmenter la précission du résultat, le diagramme pourra se fractionner en autant de sections qu'on voudra, pour lesquelles on sera libre de choisir des échelles différentes.

Le même diagramme pourrait servir à trouver une valeur approchée de l'anomalie vraie v, sans passer par l'anomalie excentrique E, car en posant—

$$\Gamma = \frac{5v + 3M}{8},$$

d'où—

$$v = \Gamma + \tfrac{3}{5}(\Gamma - M),$$

on a approximativement—

$$\Gamma = M + \tfrac{5}{4}e \sin \Gamma.$$

cette formule se déduit de la formule de M. Callandreau—

$$\operatorname{tang} \frac{5v - \mathrm{M}}{8} = C \cdot \operatorname{tang} \tfrac{1}{2}\mathrm{M}, \quad C = \frac{1 + \frac{5}{4}e}{1 - \frac{5}{4}e},$$

dont la précission est encore augmentée en prenant $C = \operatorname{tang}^2 (45° + \tfrac{5}{8}\phi)$, l angle ϕ étant donné par la relation $e = \sin \phi$. On peut juger de l'approximation que

fournit cette formule pour une série de valeurs numériques du coefficient C que j'ai calculées directement pour $e = \frac{1}{3} = 19° 5' 55''$ ($\phi = 19° 28' 17''$).

E	v	M	log C
0°	0° 0'·00	0° 0'·00	0·38051
30	41 30·40	20 27·04	0·37972
45	60 43·34	31 29·72	0·37900
60	78 27·77	43 27·61	0·37838
75	94 40·68	56 33·14	0·37811
90	109 28·27	70 54·08	0·37836
120	135 35·08	103 27·61	0·38031
150	158 32·57	140 27·04	0·38293
180	180 0·00	180 0·00	0·38414

$\log \text{tang}^2 (45° + \frac{5}{8}\phi) = 0·38060$

La formule en question est l'analogue de la formule connue—

$$\text{tang}\,(E - \tfrac{1}{2}M) = \text{tang}^2\,(45° + \tfrac{1}{2}\epsilon)\,\text{tang}\,\tfrac{1}{2}M,$$

où $\phi > \epsilon > e$, et $\sin \epsilon \cdot \sin E = \sin (e \sin E)$.

R. Radau.

Paris, Août 1883.

NOTES ON A RECENT VISIT TO SOME NORTH AMERICAN OBSERVATORIES.

By RALPH COPELAND.

Albany.

The Dudley Observatory at Albany is named in honour of the late Charles E. Dudley by his widow, Mrs. Blandina Dudley, one of its most generous supporters. Professor Lewis Boss has been the Director since 1875, and is at present assisted by Mr. R. H. Tucker.

The Olcott Transit circle has been used of late years in the observation of about 7,500 stars in the zone of Decl. + 5° to + 1° in accordance with the plan of the Astronomische Gesellschaft. The Albany zone is nearly finished and will be specially accurate, as the utmost care has been taken in the revisions.

The clock-room in the centre of the observatory has thick hollow doors, which are found very efficacious in preserving a nearly uniform temperature. To guard against the evils of shrinkage, the woodwork is made of very narrow boards. The very fine gravity clock is the work of C. Fasoldt, of Albany; it has quicksilver compensation and cost the relatively moderate sum of 300 dollars. The chronograph has a 6-in. barrel 15 inches long. The regulation is brought about by a governor, otherwise largely used in America, in which the diverging or rising balls catch against a spring that is itself retained only by stiff friction. This is thought to work better than friction against a fixed disk. In American observations the chronographic record is made by a stylographic pen in blue ink on white paper.*

At the observatory is preserved the first chronograph ever made. The records seems to have been on a spiral line on a flat disk. Here too is the Hough printing Chronograph and the Scheutz calculating machine. The nadir observations having formerly been greatly affected by the passage of trains on the neighbouring railway, Prof. Boss hit on a plan of amalgamating a *cast-iron* trough with the help of nitric acid. The process requires a good deal of patience and perseverance, but the result is eminently satisfactory as the images are not disturbed except during the transit of the heaviest trains when the ground is frozen.

The 13-inch refractor by Fitz of New York is in all mechanical respects a copy of the Dorpat Refractor by Fraunhofer. In these instruments the tube is not attached at the middle of its length to the declination axis but the end next the observer is made appreciably shorter to the greater handiness of the instrument. In these days when heavy spectroscopes and other ponderous eye-end appliances are attached to our telescopes this constructive detail ought always to be kept in view.

Prof. Boss purposes observing all the stars known to have a proper motion exceeding $30''$ in arc per century and also both components of known Binaries as far as it may be possible with the Olcott transit-circle by Pistor and Martins.

At present the Observatory is somewhat awkardly placed in being inaccessible to wheeled vehicles, but it is hoped that the proposed formation of a park in the neighbourhood may lead to the removal of this disadvantage.

Litchfield Observatory of Hamilton College, Clinton.

The 13½-inch equatoreal of this observatory although made in America is also quite in the Munich style; with it upwards of 40 asteroids have been discovered in the course of the formation of the maps which Professor C. H. F. Peters is publishing. In this work the stars are observed in declination on a mica scale, the transits being chronographed. The illuminating lamp is supported near the micrometer by a system of jointed rods. Each star, as a rule, is revised twice. The stars are laid down on the maps by pricking through finely graduated tracing or tissue paper. The reductions are simplified as much as possible and mainly worked out on the margin of the original note-book. There is also a Schröder 5-inch equatoreal on a simple stand and also one by Steinheil of 4 inches aperture to which a small spectroscope is attached. These are under special light domes. Prof. Peters possesses a small but select library containing many choice old astronomical books. The situation of the observatory is particularly fine, being some 400 or 500 feet above Clinton which is reached by a branch line from Utica on the New York Central Railroad. Here as elsewhere in the United States many of the instruments were donations by persons taking an interest in the advancement of Science. All the original records are preserved in a large fire-proof safe.

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* This plan is also used by Dr. Ball at Dunsink Observatory. Dr. Ball exhibited at the Southport Meeting of the British Association a beautiful chronograph sheet produced on this plan. The same pen is used without any confusion for the seconds and the record, the observations being always easily distinguished. At the same time all error from the parallax of two pens is avoided and the continual jerking of the pen ensures its perfect action. The spiders' webs over which the transit is taken or the designation of the star may be recorded in Morse letters and thus greatly facilitate the reading of the sheet.

Warner Observatory, Rochester, N.Y.

This consists of a handsome dwelling-house of freestone with an attached tower containing an equatoreal by Messrs. Alvan Clark and Sons of 16 inches aperture and 22 feet focus. The dome has an internal diameter of 30 feet and is thus sufficiently large to contain a large and convenient observing ladder running round on curved rails and affording ready access to the instrument in all positions without being itself in the way. An electric current passing through the rails beats the clock-seconds by means of a relay on the ladder. The mounting and tube are very plain and free from all hamper as the instrument is at present intended mainly for the search after comets and nebulæ. On the polar axis is a "live" disk driven continually by a small common clock so as to keep time with the stars; by its aid the telescope can be set at once to the right ascension of any object. The pier is 21 feet in diameter at its hollow base and 13 feet higher up where it is solid. The declination axis is 45 feet above the ground and is sufficiently high to give a clear view over the dwelling-house and almost all neighbouring objects. One structural detail is worthy of special note. The stairs from the house and the external entrance to the tower do not lead immediately into the dome but to an open air platform whence a door gives access to the telescope room. In this way the heated air from the house and the chart-room is not permitted to enter the dome to the detriment of the definition. The micrometer is of Alvan Clark's simple but practical construction with one measuring screw, the whole micrometer being itself movable by a second screw or rack-motion. The equatoreal is a gift by the citizens of Rochester to Dr. Lewis Swift in recognition of his having there discovered five comets, the orbits of which have been computed, in addition, probably, to three others that were not seen elsewhere. One gentleman is also providing a large stellar spectroscope. In connexion with the scientific facilities of Rochester it would be unfair not to mention the magnificent establishment of the Bausch and Lomb Optical Co. The great falls of the Genessee River are now made to yield an immense supply of electricity for illumination, part of which would no doubt be readily available for scientific investigations. The writer of these notes saw 1300 horse-power transmitted by one belt 54 inches in width to a series of Brush machines, and yet this represented but a very small part of the power of one of the three falls.

Dr. Lewis Swift, who hitherto has been much engaged as a hardware merchant, is about to devote the whole of his time to Astronomy.

Toronto Observatory, Canada.

This observatory is chiefly of an educational character, but it has also long been celebrated as one of the chief magnetical stations in the world, the chief magnetical quantities being continually photographed there. In this capacity it is sending out two of its staff to make a magnetic survey of the neighbourhood of the Rocky Mountains where they pass into British territory. A very complete equatoreal by Thomas Cooke and Sons has recently been erected here. C. Carpmael, Esq., is the Director.

Montreal.

The observatory belongs to the M'Gill College and is directed by Mr. C. H. M'Leod. Its principal instrument is a $6\frac{1}{4}$-in. equatoreal made originally by Fitz; the Object-glass has been completely refigured by Mr. John Clacey, of Cambridge, Mass., and now defines admirably. The mounting is very simple with 7-inch setting circles. This instrument was the gift of T. Blackman, Esq., B.A. The small transit instrument has a reticule on glass and also a long-focus collimating lens and mark. No clamp is used. The standard clock is by Howard with Denison's gravity escapement. A chronograph recording on a continuous fillet is found to be practically useless. Inside shutters of gauze allowing the free passage of air while keeping out the flies and birds struck me as being specially useful and desirable where it is necessary to ventilate rooms in which delicate instruments are used. It is proposed to take up solar photography.

Harvard College Observatory, Cambridge, Mass.

The transit circle is by Simms with an $8\frac{1}{4}$-in. objective by Alvan Clark. There are both collimators and marks with long-focus lenses the latter being some 15° or 20° below the horizontal line. The microscopes are completely boxed in with sheets of glass. Each microscope has two eye-pieces of different powers mounted on a small movable sector. For setting purposes there is a balanced arc of wood on the unused clamp-space. No clamp is used. Here again the reticule is cut on glass, the declination wires being highly inclined and thus serving the purpose of a declination micrometer.

There is also a fine broken-transit instrument made at Pulkova. Its reversing gear is so steady that Mr. Chandler actually used the instrument raised up out of the Y's, simply determining the inclination of the axis each time by the level.

In a detached building is Mr. Chandler's new equal altitude instrument which floats on quicksilver. As at present arranged it consists of a common ship's glass with a clamp in altitude and a few horizontal wires stretched across the field of view. Judging from a number of observations which had just been reduced the probable error of a single result did not seem to be above $0''\cdot2$ or $0''\cdot3$. Mr. Clacey is engaged in making a new instrument. It appears that the floating telescope can maintain a constant angle of elevation with quite as great accuracy as it is possible to determine the changes of inclination with the most delicate spirit-level.

One could not but view with extreme interest the Great Cambridge Refractor and all its belongings. Made by Merz, it is an enlarged copy of Fraunhofer's Dorpat Refractor, with some additions by Mr. Alvan Clark, among which are the conspicuous setting-circles painted white, and the great driving sector impelled by a steel ribbon pulled in turn by the clock. On a marble slab on the wall are eighty-three names of the original contributors to the telescope, that of J. Quincey Adams among them, and elsewhere is the statement that "this tower was the gift of Hon. David Sears." The dome runs on balls which keep their relative positions for several years. In a recess were many of the accessories, *e.g.* the Photometers *H*, *I* and *J* which although only a few years old have already become quite classical from the amount of work done with them. There was also a short Pritchard wedge preceded by a line, so that by noting first the transit over the line and then the instant at which the star disappeared in the gradually darkening wedge, the star's brightness became known. There too was the camera with ordinary glass lenses used for photographing the stars, for which purpose it is mounted on a simple equatoreal stand driven by clock-work. There was also a beautiful star spectroscope by Hilger, with the collimator and viewing telescope each of about 9 inches focal length. In front of the slit was a movable total-reflecting prism on Prof. Winlock's plan, which served as a finder and also for viewing the object under study with almost the full power of the instrument. Professor Smyth's prismatic pointer had also been pressed into the service. It was apparently intended to use only one prism at a time. All the measuring was performed by a long screw working in a nut and not on a ratched sector.

In another part of the observatory is the meridian photometer by which any star as it comes to the meridian is compared in brightness with the pole-star. Much of the simpler routine computation is done by four lady assistants, who both here and elsewhere in the United States are found to be most untiring and conscientious. The original records are kept near the door to afford a greater chance of being saved in case of fire. Much attention has been devoted to simplifying the self-imposed tasks of the observatory ; to give a single instance, the bound telegraph forms relating to the discovery of asteroids are arranged in sets on pink paper, the sets being separated by a plain white leaf. As is well known Harvard College is now the central American station for the receipt and despatch of telegrams relating to astronomical discoveries. Recently Professor Pickering has been endeavouring to organize amongst amateur astronomers a society for the observation of variable stars.

For books not of a purely astronomical character the observatory is dependent on the magnificent Public Library of Boston which now contains about 300,000 volumes. So efficient is the management that at 10 A.M. on August 15th books received on the 13th were readily accessible without special request or trouble.

At the optical works of Messrs. Alvan Clark and Sons at Cambridgeport, the writer saw the 38-in. disk of glass destined for one of the lenses of the great Lick telescope. The largest lens in hand was a 12-in. intended for the new equatoreal at West Point. A trial of this in a somewhat hazy sky showed extremely clean and symmetrical images with rather less blue in the secondary spectrum than is usually found in European lenses. All the object-glasses made by the renowned American firm are tested by reflexion from a vertical plane of glass that in itself is a masterpiece of optical workmanship. Otherwise in comparison with workshops on this side of the water the appliances were very few and comparatively rude, the conditions of success being sought in skilful manipulation and personal supervision rather than in the precision of exact mechanism. The establishment thus resembled the laboratory at Birr Castle more than any of our great optical workshops. Although in his 80th year the head of the firm still takes an active part in the work and showed with evident pride a self-acting fire alarm of his own construction, and later on, without the aid of a finder, picked up a telescopic star very awkwardly near the zenith with a skill that many a young astronomer might have envied. He also related how the late Rev. W. R. Dawes was at one time his best client and at the same time one of the first of our highly skilled observers to appreciate an American object-glass. The huge temporary equatoreal mounting and tube with which the Pulkova 30-in. lens was tested are still in position, as they will serve a like purpose for the Lick object-glass. With the Russian glass a close companion was discovered to ζ Geminorum.

New Haven.

The Winchester Observatory of Yale College has a Transit-circle of 3·8-in. aperture, originally made by Ertel and Sons, but almost remodelled in the United States. There is also a Transit instrument of 4-in. aperture by Troughton and Simms for which the roof shutter lifts clean off to one side on Mr. Grubb's plan as arranged for the Cork Observatory. Besides a number of smaller instruments there is a very stable 8-inch equatoreal by Grubb and also a 6-inch Heliometer by Messrs. Repsold fitted up with all the elaborate refinement for which that firm is so favourably known. To avoid tremor the driving-clock is detacted from the pier and is founded on the wall of the tower. It is regulated by a steel pendulum, the circular motion of which is permitted and restrained by the elasticity of the rod of the pendulum itself which is secured as firmly as possible at its upper end. Four steel rods at the eye-end actuate the clamps and slow-motions in right-ascension and declination. The cylindrical scales of the objective's halves which move in the surface of a cylinder, and their metallic thermometer are read off in different parts of the field of the same telescope-microscope, and in the same way the declination and position circles are both read off in two places by one pair of tubes. These and of course the delicate focus scale are all readable from the eye-end. As from the very nature of the instrument the whole of the telescope proper rotates in position, many of the accessories, *e.g.* the finder, are carried by an external tube which does not rotate. By putting the various handles and eye-pieces nearly in one plane they are all readily accessible without being mutually in the way. The large hour-circle is ratched all round. These particulars are but intended to give a general idea of the instrument which will doubtless soon meet with the full description it deserves. The dome, beginning with the inside, is covered with wire-netting, canvas, felt and lastly painted canvas outside; it has a large segmental opening. Besides the regular astronomical work of the observatory there is a department for the testing of watches and supplying time by clocks, regulated automatically every hour by J. A. Lund's patent, and another department in which thermometers are systematically tested. The staff consists of Dr. Leonard Waldo, Mr. Wilson in charge of clocks (five in number), watches and the equatoreal, Mr. Sherman at the thermometers, Mr. Kershner as observer, principally with the heliometer, and lastly Mr. Brown as Secretary.

Princeton, New Jersey.

At Princeton are two observatories; the older establishment is that of the J. C. Green School of Science fitted up mainly for educational purposes, and possesses accordingly a number of comparatively small instruments, among which are a 3-inch Transit instrument in the meridian and one in the prime vertical, as well as a zenith telescope of the same aperture and a 4-inch Transit circle with two 30-inch circles. A fine sidereal clock by Howard and Co., of Boston, fitted with Professor Young's detached escapement controls a clock in each of the observing rooms by a second's current. One of Mr. Browning's 9-inch reflectors on an altazimuth stand serves for experiments, but the chief instrument is a 9½-inch Alvan Clark equatoreal with the objective constructed on Gaussian curves. The definition has been shown by numerous observations to be about as nearly perfect as possible. At present it is used on the sun with one of the large curved gratings ruled by Professor Rowland, of Baltimore, on speculum metal.

The second or Halsted Observatory consists of a remarkably fine and lofty dome 45 feet high inside, surmounting a handsome building. The dome, built some time ago, is so heavy that it is itself turned round and its 6-foot wide shutter moved up and down by a four H.P. gas engine, the work of Schleicher, Schrumm and Co., of Philadelphia. This powerful adjunct drives an Edison dynamo and besides furnishing a powerful current for experimental purposes, excites a number of Edison's incandescent lamps. The grand 23-inch Alvan Clark equatoreal of 30 feet focus was provided by subscriptions on the part of friends of the Princeton University. At the eye-end it terminates in a very strong brass barrel some 8 inches in diameter to the outside of which are attached either of the gigantic spectroscopes with which Professor Young carries on his observations One of these is on the reflex plan, the same telescope of some 5 inches aperture serving as collimator and viewer, the optical axis being slightly tilted. The other spectroscope has a 3-inch direct-vision prism and is furnished with a coil and comparison apparatus. Both spectroscopes are furnished with carriages in which they rest when not in use and which are provided with suitable lifting machinery to facilitate the attachment to the great equatoreal The lenses of the object-glass are about 7-inches apart, so that the inner surfaces can be wiped without disturbing the adjustments, which at present are under control from the eye-end so that the centering can be varied while looking at an object. A special shade shelters the astronomer when viewing the sun, while an ingenious water circulation cools the screens that protect the eye-piece. Quicksilver is used to relieve the pressure on the polar-axis. The sector is here also driven by steel bands on the favourite American plan. The observing chair and its roomy eye-piece drawers is retained at any height on a straight incline by the friction of a cord passing twice round a block of wood at the back of the seat. The chair can be lowered by pulling on the cord without rising, but to elevate the chair it is requisite to stand on a side ladder. The great excellence of the equatoreal has been shown by the recent determination of the ellipticity of Uranus and the detection of belt-markings on that planet.

Noteworthy is a simple but efficient gas arrangement. Alongside of the burner a secondary pipe constantly maintains a small sheltered flame which lights the gas when turned on. At the other observatory some of the jets had electrical apparatus for effecting the same end. In consequence of the vacation here as at Cambridge the writer was so unfortunate as to miss seeing the Director of the observatory.

The United States Naval Observatory, Washington.

The great 26-inch equatoreal differs only in size from the rest of the telescopes made by Messrs. Alvan Clark and Sons. It has the same somewhat light mounting and axes, the same plain well-proportioned tube of riveted steel plates and the same absence of nearly all the outer fittings and adjuncts that are considered desirable about large instruments in Europe. It is indirectly driven by water which continually winds up the clock-weight. The clamping in declination is done in the usual way with a long rod, but both the slow motions, the clamping in right ascension and a special quick motion in hour angle are effected by cords. The white setting-circles are lit up when required by gas-light reflected from the wall by concave glass mirrors. The setting is done by the attendant who manages the dome and also throws light from a bull's-eye lantern into the end of the micrometer when bright-wire illumination is desired. The parallel wire micrometer has but one measuring screw as with the considerable focal-lengths of our large instruments it is quite possible to construct micrometer screws that are practically perfect and thus do away with the necessity of changing the zero-point to eliminate periodic error. The sidereal clock is let into the wall of the dome so as to be out of the way. In short everything is done to give free scope to the observer and to simplify the instrument, trusting to skill on the astronomer's part and considerable zeal on that of the attendant to meet the ever-varying demands of the work in progress.

On the evening of August 18, 1883, Professor Hall had the great kindness to show the writer a number of objects through the telescope. α Lyræ with powers of 400 and 600 exhibited a sharp central disk with fine brilliant uniformly distributed rays on a moderate ground of outstanding palish blue ; of course the well-known companions were egregiously conspicuous. The action of the lens did not strike me as being different from that of the best telescopes with which I was acquainted. ϵ Lyræ was looked at chiefly as being a familiar object and one that the writer had just seen through a 12-inch Alvan Clark glass. The moonlight and some hazy cloud doubtless prevented the gain in light from being specially evident. Σ 2422 with a central distance of about $0''\cdot8$ gave an opportunity of making a demand on a power of 900. A sketch repeatedly compared with the object shows the disks to have been about $0''\cdot4$ in diameter and consequently separated by that interval. OΣ 387 with a distance of about half a second of arc was so distinctly separated as to give the assurance that under very favourable circumstances the glass might be trusted to work down to the closest theoretical limits of rather less than two-tenths of a second of arc provided the component stars were fairly equal and not too brilliant. Mr. Burnham's companion to β Aquilæ was so obvious that it could not be considered a test object. The instrument was very steady but yielded in an elastic manner to the extent of one minute or so of arc when lightly pressed at right angles to the meridian, thus indicating a want of transverse stiffness in the mounting. The attendant did the setting quickly by the aid of the coarsely divided painted circles, Professor Hall dictating the hour angles and the polar-distances. The equatoreal stands on the main floor of the observatory in a comparatively lightly constructed building with the usual hemispherical dome moved on a plan little differing from that of the Dublin dome. The opening is 6½ feet wide and is covered by a sheet of oiled canvas running in guides and let down in a nautical fashion when the telescope is in use. The special computing room for the equatoreal work adjoins the dome. The cost of the instrument was about £9,000, of which about £1,200 were paid to Messrs. Chance Brothers and Co., of Birmingham, for the glass disks.

Mounted in a dome surmounting the whole observatory is a 9·6-inch Munich equatoreal of the usual construction. This, the fine Mural circle by Troughton and Simms, as well as the 7-foot transit by Ertel do not call for special remark. The great Transit-circle by Pistor and Martins has a telescope of 12 feet focal length and 8½ inches aperture with interchargeable ends. The collimators are of the usual small German type having only 2⅛ inches aperture, so that it may well be doubted whether the results they give fairly represent the collimation of the main telescope. The two movable circles have an effective diameter of 43·4 inches and are divided to every 2′, each being read by four microscopes. The instrument is arched over by ladders, much on the Greenwich plan, so as to afford every facility for reflex and nadir observations. Professor Newcomb's researches show that the graduations of the circles are remarkably free from systematic errors and that greater final accuracy is to be expected from occasionally changing their positions so as to use fresh divisions, than by the most elaborate determination and application of division corrections. The walls of the building are of thin sheet iron shaded by louvre work on the outside. The inside temperature is found to differ very little from that of the external air. In a small detached building is a new reversible transit instrument made by Messrs. Stackpoole Bros., of New York, from designs by Professor Harkness. In it the bearings are segments of cylinders—a detail of construction worthy of more general imitation.

All the photographs of the recent Transit of Venus taken by the American parties are now being measured at the Naval Observatory under the supervision of Professor Harkness who has devised most ingenious apparatus for the purpose. Polar coordinates are measured and referred to the photographed image of a plumbline whose supports were regularly varied to eliminate *kink.* Between 1300 and 1400 pictures are available. They were taken with long-focus lenses through which the light was thrown from unsilvered mirrors, both being made by the Clarks. The writer saw some spirit-levels of a novel construction in which the tube was enclosed in close proximity to the glass lid of a box ; the divisions being on the underside of the covering and not on the tube, Professor Harkness being of opinion that the figure and elasticity of the tube are injured by the ordinary divisions.

Conclusion.

The writer, having enjoyed the singular good fortune of seeing through a number of the finest telescopes in the world, hopes that it may not prove altogether uninteresting if he places on record his general impressions as to the actual state of telescope construction on both sides of the Atlantic. First as regards their optical merits : it does not seem that any material difference as to the mere power of separating close double stars exists in the object-glasses made by the chief opticians in Europe and America. On a night of good definition any of their telescopes may be trusted to divide a fairly equal pair of stars at a distance indicated by Dawes's* table, of which the following is a sufficient specimen :—

Aperture in inches.	Least separable distance.	Aperture in inches.	Least separable distance.
1·0	4″·56	12·0	0″·380
2·0	2·28	15·0	0·304
3·0	1·52	20·0	0·228
4·0	1·14	25·0	0·182
6·0	0·76	26·0	0·175
8·0	0·57	27·0	0·169
10·0	0·46	30·0	0·152

We thus see that in this respect our telescopes are practically perfect and also that the atmosphere on the very best nights is sufficiently steady to permit their full power to be used. If however we test them on double stars of which the components differ very much in brilliancy, then it is by no means so easy to come to a certain conclusion. There is the secondary spectrum to contend with, respecting the character of which it may be said that a certain degree of personal taste or fashion exists. Some persons, notably opticians, seem to be little disturbed by a decidedly blue glare while others prefer a wine-coloured fringe. Perhaps, indeed probably, there is a physiological difference in the observers, for if we suppose a person to be blind to the extreme blue and the violet rays only of the spectrum ; to him an over-corrected object-glass would be perfect. With it he would be able to make out the closest companions of blue stars or to see comparatively faint ones right up to the moon's bright limb. To such a person however an object-glass under-corrected to the same extent would appear to be a decidedly bad one. To the writer (as well as to many of his colleagues) an average glass by Cooke or Grubb, and to a less extent by Clark, appears over-corrected, while one of Schröder's and some of the Munich glasses are under-corrected. But here an important practical difference enters into consideration that has been particularly experimented on by Mr. Russell, of Sydney, viz., that the correction of an object-glass may be lessened by separating the lenses, hence an over-corrected object-glass may be adjusted to any desired extent while one that is under-corrected can only be used in the state in which it left the maker's hands. As an example it may be mentioned that the somewhat over-corrected object-glass of the 15-inch equatoreal at Dun Echt has been materially improved by separating its lenses $\frac{1}{8}$ of an inch, while a separation of $\frac{3}{10}$ of an inch was found to throw out too much red around the primary image. This degree of improvement is best shown by the extremely linear character of the spectra of stars which it now gives. But in this connexion it is only fair to mention that in making this object-glass Mr. Grubb was tied down to the relatively short ratio of 12 to 1 between the focal length and aperture. Opticians have not neglected to avail themselves of this property and accordingly we find three of the largest objectives in the world, viz., the 27-in. at Vienna by Grubb, the 23-in. at Princeton and the great Russian 30-in., both by Alvan Clark and Sons, with their lenses separated by a considerable interval. Assuming a large lens to be made of satisfactorily good disks and having its curves and interval so adjusted as to give the best attainable results, there is another detail of construction that demands increased attention with every augmentation of size, *i.e.*, the state of the surfaces of the lenses. Formerly it was too readily assumed that provided the curves were right a few scratches more or less did not matter. There is a well-known story of an optician who on being blamed for turning out a badly scratched lens replied that an object-glass was to be looked through and not at. The optician was nevertheless in the wrong, for if delicate objects are under observation, no matter whether these are small companions of large stars, or minute satellites of bright planets, there can be no doubt that the finish of the objective plays a considerable part in their visibility. Nor is it merely necessary that the surfaces should be correctly formed and well polished; it is also requisite that they should be kept scrupulously clean and, above all, free from grease, the slightest trace of which when spread over a lens must throw out irregular diffraction spectra that will materially affect the visibility of any small point of light in the neighbourhood of a brilliant object. In this respect no practical astronomer should neglect to assure himself that an object-glass is really doing full justice to the maker.

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* Mem. R. Astr. Soc., Vol. 35, pp. 158–159.

It still remains to say a few words about the mountings of large equatoreals. In America, as we have seen, the mounting is just or barely sufficient to permit of a satisfactory use of the grand optical powers of their larger instruments and no refined detail of auxiliary apparatus is attempted. On the continent we find the convenience of the astronomer studied in the most painstaking manner and perhaps in no instruments in the world is this so carefully kept in view as in the finer German instruments. This is doubtless due in no small measure to the intimate relations which exist between the chief continental instrument makers and practical astronomers, so that just that kind of apparatus is provided which experience has shown to be requisite. On the other hand in the stability and rigidity of their mountings the larger English and Irish instruments stand pre-eminent, while they year by year show a greater variety of really available subsidiary apparatus. Indeed there can be little room for doubt that the elder Grubb by his elegant arrangements for relieving the friction of both axes of the equatoreal mounting practically removed all limits to its size and strength, while in the little known 25-inch instrument at Gateshead, by Cooke and Sons, we have a telescope that only requires to be efficiently used in a good atmosphere to show its great merits in all respects.

In conclusion, whether we take large European or American instruments, the prospect is most encouraging both to the astronomer and the instrument maker. Nowhere can signs be detected that the utmost practical limit has been reached. A twenty-seven inch glass can be managed with probably greater facility now than a ten-inch fifty years ago and with something closely approaching to the full gain in power due to increased size. The question of size now as then reduced itself to the production of suitable disks of glass and to that of cost. Here it is that the silvered glass reflector has offered facilities of which several distinguished investigators have not been slow to avail themselves.

RECENT LITERATURE.

Publicationen des astrophysikalischen Observatoriums zu Potsdam. Dritter Band, Potsdam, 1883, 4to. (307 pp. and seven plates). This volume contains: Observations and researches on the physical constitution of Jupiter and Mars by Dr. O. Lohse; Researches on the mass of Jupiter by Dr. P. Kempf; Spectroscopic review of the stars down to 7·5 mag. in the zone − 1° to + 20° Decl. by Prof. Vogel and Dr. G. Müller; Photometric Researches by Dr. Müller; Pictures of Sun Spots, with notes on astronomical drawings and their reproduction, by Dr. O. Lohse.

The observations of Jupiter were made by Dr. Lohse with the 8-inch Refractor by Grubb and the 11-inch by Schröder from 1878 to 1881. A great number of woodcuts and twelve lithographs serve to illustrate the notes. The great red spot was carefully observed, especially the distance of its ends and centre from the East limb. The time of rotation of the spot does not appear to have been quite constant during the four years it was observed. The rotation of a white cloud in the equator-zone was also determined and found to be about $5{\cdot}5^m$ less than that of other surface markings in various jovicentric latitudes, so that the white cloud must have maintained for at least three years a relative motion of 115 meter per second. Already Cassini and Schröter found the rotation of equatoreal formations to be $9^h\ 50^m$, and the difference between this result and that derived from spots in higher latitudes deserves great attention. Dr. Lohse has taken a number of photographs of Jupiter with the 11-inch Refractor, using the gelatino-bromide process, and the results he has deduced for the dimensions of the planet seem promising for the application of photography in this direction. The observations of Mars were made in 1879 on seven nights, but others from 1873 and 1877 were also made use of in compiling a map on Mercator's projection.

In the years 1868 to 1870 Dr. Vogel made a number of observations of differences of R.A. between Jupiter and the two outer satellites, which have now been utilized by Dr. Kempf to determine the mass of the planet. A similar series of observations of the fourth satellite was made in 1832–36 by Airy, but as their reduction was made before the publication of Damoiseau's improved theory of the satellites, Dr. Kempf has reduced them anew. At the end of the paper he puts together the following table of results for the mass of Jupiter :—

I. *From observations of the Satellites*—		
1. Heliometer observations :	Bessel (Schur),	1048·629 ± 0·134
	Schur,	1047·232 ± 0·246
2. Transit observations :	Airy (Kempf),	1047·641 ± 0·488
	Vogel (Kempf),	1047·767 ± 0·310
II. *From perturbations*—	Hansen (Egeria),	1051·12 ± 0·81
	Becker (Amphitrite),	1047·37 ± 1·31
	Möller (Comet Faye)	1047·788 ± 1·185
	Krüger (Themis),	1047·538 ± 0·192
	Asten (Encke's Comet, 1865–71),	1047·611 ± 0·171
	Dubjago (Diana),	1045·25 ± 0·46

The third paper contains Dr. Vogel's spectroscopic review of the stars down to 7·5 mag. in the zone − 1° to + 20°, made from January 1880 to March 1882 with the 11-inch Refractor and the well known small spectroscope devised by him in 1873. 4051 stars were observed and their colour and the general character of their spectra are given in tabular form.

The fourth paper contains photometric observations by Dr. G. Müller with a Zöllner photometer. The instrument is first carefully examined and the curve of intensity (see above p. 36) determined, after which the Author proceeds to discuss his observations on the extinction of light in the atmosphere. This subject was investigated by Seidel, who from his observations of all the stars of the first magnitude deduced a table of mean extinction which he afterwards tested by comparing 208 stars from the first to the fifth magnitude *inter se*. Partly because this method does not clearly prove whether the colour of the star has any influence on the extinction, partly because the reductions are very troublesome, Dr. Müller preferred to compare five stars (α Aurigæ, α Cygni, η Ursæ majoris, δ Persei and α Tauri) at different altitudes with the Pole Star. As to the influence of colour, the Potsdam observations have not yet given decisive results, though they seem to indicate that the light of the red star α Tauri is most and that of the white stars least reduced by extinction. For zenith distances greater than 80° the Pole Star was not used, but the objects (chiefly planets) were observed as often as possible when rising or setting, and the extinction deduced from the differences between consecutive readings of the intensity circle. The resulting table of extinction is in extremely good accordance with Seidel's. Finally the Author compares his results with Lambert's theory and finds that they agree with the theory nearly down to the horizon, if the desired accuracy is not greater than 0·1 magnitude.

UEBER EINE METHODE DIE POLHÖHE MÖGLICHST FREI VON SYSTEMATISCHEN FEHLERN ZU BESTIMMEN.

Von Professor J. C. KAPTEYN.

I. *Einleitung.*

Es ist eine sehr bekannte Sache, dass zwischen den Resultaten für die Declinationen der Fundamentalsterne, welche an verschiedenen, auch der best ausgestatteten Sternwarten, erhalten werden, Unterschiede bestehen, die weit ausser den Grenzen liegen, welche man, in Rücksicht auf die Uebereinstimmung der einzelnen, an der nämlichen Sternwarte angestellten Beobachtungen, erwarten müsste. Diese Erscheinung ist vermuthlich dem Zusammenwirken mehrerer Ursachen zuzuschreiben, und es verdienen hier wahrscheinlich das Mangelhafte unserer Kenntniss der Strahlenbrechung und der Biegung des Rohres und der Kreise in erster Linie genannt zu werden. Besonders auch in den letzten Jahren hat diese Sache eine Menge sehr interessanter Untersuchungen hervorgerufen. Einerseits werden neue und möglichst einwurfsfreie Mittel zur Bestimmung dieser Factoren aufgesucht, oder genaue Theorien entwickelt; andererseits bezweckt man das Eröffnen neuer Wege, die zur Bestimmung der Declinationen führen, von jenen Fehlerquellen aber ganz oder theilweise unberührt bleiben. Ein Versuch dieser letzteren Art soll in diesem Aufsatze gemacht werden.

Schon im Anfang des Jahres 1880 hatte ich mir die hier folgende Methode in rohen Umrissen entworfen, in der Hoffnung bald wenigstens über ein gutes Universalinstrument verfügen zu können. Diese Hoffnung hat sich leider bis jetzt nicht verwirklicht, und ich konnte mich nicht entschliessen öffentlich mit einem Vorschlag hervorzutreten ohne zugleich durch Beobachtungen die Brauchbarkeit der vorgeschlagenen Methode beweisen zu können. Ich verdanke es dem Herrn Professor Bakhuyzen, dass ich jetzt diesen Beweis, wenigstens was die zufälligen Fehler betrifft, glaube führen zu können. Mit grösster Bereitwilligkeit hat mir Herr Bakhuyzen während einiger Wochen des Sommers 1882 den Gebrauch des Repsold'schen Universalinstruments der Sternwarte zu Leiden eingeräumt. Ungeachtet der ungünstigen Witterung während dieser Zeit (21 Juli—18 Aug.) gelang mir eine Zahl von Beobachtungen, welche, wie ich glaube, beweisen, dass bei sehr ungünstigen Umständen und mit einem Instrument von geringen Dimensionen, nach dieser Methode in kurzer Zeit eine sehr befriedigende Genauigkeit erreichbar ist. Es sei mir erlaubt Herrn Director Bakhuyzen und seinem Bruder, dem Observator der Sternwarte, hier öffentlich meinen Dank für ihre Freundlichkeit und für manche Bemerkung, die ich im Folgenden benutzt habe, abzustatten.

Die Schwierigkeit, die man bei der Bestimmung absoluter Declinationen zu überwinden hat, ist eine doppelte: die Bestimmung der Polhöhe frei von allen systematischen Fehlern, und die fehlerfreie Bestimmung der Declinationen, sobald die Polhöhe bekannt ist. Es wird hier fast ausschliesslich nur der erste Punkt behandelt werden, obgleich man die hier vorgeschlagene Methode kaum anders betrachten kann als ein Mittel um zur Kenntniss der absoluten Declinationen zu gelangen, und die Bestimmung dieser letzteren sich auch ganz natürlich anschliessen lässt; denn auch so wurde die Ausdehnung dieses Aufsatzes eine unerwünscht grosse, überdies sind, nach meiner Meinung, die Schwierigkeiten des zweiten Punktes viel geringere. Zur Begründung dieser Meinung nur die folgenden Andeutungen.

Sobald die Polhöhe bekannt ist, kann man mehrere Wege angeben, die zu einer voraussichtlich sehr fehlerfreien Bestimmung von absoluten Declinationen führen werden. So wird man die Declinationen bestimmen können;

(*a*) von Sternen nahe am Pol mittelst der Beobachtung des grössten Azimuths. Diese Bestimmung ist nur in geringem Maasse von der Polhöhe abhängig.

(*b*) von Sternen die in geringer Entfernung südlich* vom Zenith culminiren, mittelst Beobachtungen im ersten Vertical.

Wird nun angenommen, dass die Strahlenbrechung in gleicher Zenithdistanz nördlich und südlich den gleichen Betrag hat, eine Annahme die auch bei der Polhöhenbestimmung gemacht werden muss und daher noch näher besprochen werden soll, so werden mittelst Beobachtungen kleiner Höhenunterschieden im Meridian (z. B. nach der Talcott'schen Methode) bekannt:

(*c*) die Declinationen von Sternen, die bei ihrer Culmination südlich vom Zenith gleiche Höhe erreichen mit den unter (*a*) genannten;

(*d*) die Declinationen von Sternen, die bei ihrer Culmination nördlich vom Zenith gleiche Höhe mit den unter (*b*) genannten erreichen;

(*e*) die Declinationen derjenigen südlichen Sterne, die im Meridian gleiche Entfernung vom Scheitel erreichen als die unter (*b*) und (*d*) genannten Sterne bei ihrer unteren Culmination.

Geht man z. B. für eine Polhöhe von + 60° bei den Beobachtungen (*a*) bis zu 10° Poldistanz; und bei (*b*) bis zu 2° Zenithdistanz, so können die folgenden absoluten Declinationen bestimmt werden—

(*a*)	$\delta = +90^\circ$	bis	$+80^\circ$
(*b, d*)	$+62$	–	$+58$
(*c*)	$+40$	–	$+20$
(*e*)	$+2$	–	-2

In der nördlichen Hemisphäre bleiben demnach nur drei Gürtel, wovon jeder 18° breit ist, unbestimmt; diese werden sich ohne viel Mühe überbrücken lassen.

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* Es wird hier und in der Folge der Beobachter in der nördlichen Hemisphäre gedacht.

Die hier unter (*c*) genannten Sterne werden durch die nämlichen Beobachtungen bestimmt, auf welchen auch die Polhöhenbestimmung beruht. Werden also zu den Beobachtungen, die zur Polhöhenbestimmung erforderlich sind, nur noch die Beobachtungen (*a*) hinzugefügt, so gelangt man zugleich zur Kenntniss der Declinationen dieses Gürtels von südlichen Sternen, für dessen Mitte $\delta = 2\phi - 90°$ ist. Es ist ein wichtiger Umstand, dass dieser Gürtel für die vorzüglichsten Sternwarten der südlichen Hemisphäre ganz oder theilweise mit dem analogen Gürtel der bedeutendsten nördlichen Sternwarten zusammenfällt (z. B. Cordoba, Cap einerseits und Pulkova andererseits, ebenso Melbourne und Greenwich, Leiden, Berlin u.s.w.) Man kann also schon durch Beobachtungen an nur zwei wirklich bestehenden Sternwarten Nordpol und Südpol in Verbindung setzen, wodurch eine schöne Controle für die erreichten Resultate gewonnen wird, zumal weil, wie dem weiter unten folgenden zu entnehmen ist, die Genauigkeit, welche man für die Polhöhe der südlichen Sternwarten erwarten kann, so ziemlich derjenigen gleich ist, die man für die weiter vom Equator entfernten, nördlichen Sternwarten finden wird.

Nach dieser Abschweifung kehre ich zu meinem eigentlichen Gegenstande zurück. Um dem Ziel der ganzen Untersuchung—Unabhängigkeit von systematischen Fehlern—möglichst nahe zu kommen, war ich vor Allem bestrebt der Methode eine fast vollkommen symmetrische Form zu geben; die Kleinheit der zufälligen Fehler konnte dabei erst in zweiter Linie in Betracht kommen. Dennoch glaube ich, dass auch in dieser Hinsicht diese Methode sich nicht unvortheilhaft ausnehmen wird. Die vorausgesetzten Beobachtungen sind zweierlei Art. Beide können mit einem Altazimuthe angestellt werden:

(*a*) *Unterschiede* in Zenithdistanz von Sternpaaren bei ihrer Culmination in nahezu gleicher Entfernung nördlich und südlich vom Zenith und zwar in einer Höhe, die nie beträchtlich kleiner ist als die Polhöhe des Beobachtungsortes.

(*b*) *Unterschiede* in Azimuth von Sternen, deren Declinationen wenigstens innerhalb ein Paar Grade einander gleich sind und die unmittelbar nach einander, in gleicher Zenithdistanz und also in fast gleicher Entfernung Ost und West vom Meridian, beobachtet werden.

Wo noch andere Grössen in Anwendung kommen, wird immer nur eine genäherte Kenntniss vorausgesetzt.

Schon hieraus wird man grösstentheils übersehen, in wiefern man eine grosse Unabhängigkeit von systematischen Fehlern zu erwarten berechtigt ist. In Betreff der Refraction beruht die Richtigkeit der Methode auf zwei Voraussetzungen: erstens, dass die Refractionen in gleicher Entfernung nördlich und südlich vom Zenith einander gleich sind (oder wenigstens bis zur ZD = Colatitude nicht systematisch verschieden), und zweitens, dass systematisch einwirkende seitliche Refraction nicht zu befürchten sei. Dass wirklich aus diesen Quellen gar keine systematische Fehler entspringen werden, ist a priori wenig wahrscheinlich; noch weniger wahrscheinlich aber ist meines Erachtens, dass es je gelingen wird bei der Bestimmung der Polhöhe beide Fehlerquellen gänzlich zu vermeiden, und man wird sich damit zu begnügen haben, ihren Einfluss möglichst klein zu machen. Es ist leider in Betreff dieser Sachen so wenig bekannt, dass sich nicht viel bestimmtes davon behaupten lässt. Von den zwei Voraussetzungen ist wohl die letztere im Allgemeinen die weniger gefährliche. Erstens nämlich ist zu erwarten, dass (wenigstens in solchen Höhen, welche bei diesen Beobachtungen vorkommen) die laterale Refraction viel unbedeutender sein wird als der Unterschied der nördlichen und südlichen Strahlenbrechung, dann aber hat man es in seiner Hand, an *einer* Sternwarte mehrere Polhöhenbestimmungen zu machen, bei welchen man in ziemlich verschiedener Entfernung vom Meridian und vom Zenith zu beobachten hat; hierdurch wird nicht nur der ganze Betrag der seitlichen Strahlenbrechung, sondern auch der Factor, mit welchem multiplicirt diese in dem Ausdruck für die Polhöhe erscheint (und der ohnehin schon selten über 0·5 oder 0·6 hinausgehen wird) verschieden ausfallen. Schliesslich wird voraussichtlich bei dem fortwährenden Verrücken des Daches, das bei diesen Beobachtungen unumgänglich ist, die Temperaturvertheilung im Beobachtungslocal eine viel weniger systematische sein. Auf Beobachtungen gestützte Untersuchungen über laterale Refraction bei Sternbeobachtungen gelang es mir nicht aufzufinden.

Bei der Besprechung der ersteren der beiden Refractionsanomalien sind Untersuchungen über constante Unterschiede der Refraction in gleicher Distanz zu beiden Seiten des Zeniths, sowie über den Einfluss eines Unterschiedes der äusseren und inneren Temperatur, in Betracht zu ziehen. Untersuchungen über den Einfluss der Tageszeit auf die beobachteten Zenithdistanzen stehen hiermit in innigstem Zusammenhang, denn der genannte Unterschied der Temperaturen ist meistens von verschiedenem Vorzeichen bei Beobachtungen die bei Nacht und bei solchen die bei Tag (wenigstens während des Sommerhalbjahrs) angestellt werden. Der sicherste Weg bei den Untersuchungen ersterer Art ist wohl derjenige, der von einer Vergleichung der Oerter von Sternen, welche in den beiden Halbkugeln beobachtet sind, ausgeht. Werden die Oerter mit der aus Beobachtungen der Circumpolarsterne hervorgehenden Refraction reducirt, so wird man die Unterschiede der gefundenen Oerter einem Unterschiede der Strahlenbrechung in den beiden Hälften des Meridians zuschreiben können. Nach der Theorie des Herrn Gyldén ist ein solcher Unterschied a priori als sehr wahrscheinlich zu bezeichnen (wenn auch quantitativ vielleicht sehr gering) und die Folge geringer Neigungen der Luftschichten gegen die zur Richtung der Schwere senkrecht gelegten Ebenen. Die Entwicklung der Theorie führt den Verfasser zu dem Schluss, das die Correctionen der auf gewöhnliche Weise berechneten Refraction in kleinen Höhen sehr nahe den Quadraten der totalen Refractionen proportional sind. In kleinen Zenithdistanzen wird daher dieser Unterschied unbeträchtlich, und wenn man die Oerter von Sternen, die an einer Sternwarte in mässigen Zenithabständen beobachtet sind, mit den Oertern vergleicht, die für dieselben Sterne an einer Sternwarte der entgegengesetzten Halbkugel aus Beobachtungen in sehr grossen Zenithabständen gefunden sind, so werden die Unterschiede, wofern sie der hier besprochenen Ursache zuzuschreiben sind, mit diesen letzteren Zenithdistanzen nahe im Verhältniss von $\mathrm{tang}^2 z$ wachsen müssen.

Von einer derartigen sehr raschen Zunahme zeigen aber die wirklichen Unterschiede zwischen den Bestimmungen nördlicher und südlicher Sternwarten höchstens Spuren. Man vergleiche z. B. die in der V.J.S. IV. p. 104-108 aufgeführten Unterschiede zwischen Greenwich-Pulkova und Melbourne, ebenso die für grosse Zenithdistanzen in Leiden gefundenen Unterschiede Cap—Leiden und Melbourne—Leiden.* Die ersten Differenzen sind von Herrn Stone (Month. Not. xxviii. p. 32) wirklich einem Unterschiede der nördlichen und südlichen Refractionen in Melbourne zugeschrieben. Professor Gyldén hat aber gezeigt (V.J.S. xi. p. 183), dass diese Differenzen sich noch besser durch die Voraussetzung erklären lassen, dass die in Melbourne angewandte Biegungs-Constante zu gross ist. Durch diese Voraussetzung wird sogar der Unterschied Greenwich—Melbourne im Mittel aus 13 verglichenen Sternen zwischen 40° und 60° N.P.D. (also für Z.D. zwischen 68° und 88°) fast zum Verschwinden gebracht. Jedenfalls ist es klar, dass blos durch Anbringung von Correctionen, die in breiten Gürteln der Declination constant sind, und selbst in kleinen Höhen, die Unterschiede sehr unbeträchtlich werden ; und es erscheint die Behauptung nicht zu gewagt, dass die Unterschiede, die einer Ungleichheit der nördlichen und südlichen Refractionen zugeschrieben werden könnten, selbst für Zenithabstände zwischen 80° und 85° den Werth 1″ nicht oder wenigstens nur sehr wenig überschreiten. Dieses verbunden mit der Annahme, dass diese Anomalie in kleinen Höhen dem Quadrate der Refractionen nahezu proportional sein muss, lässt schliessen, dass in mittleren Entfernungen vom Scheitel, z. B. für Z.D. kleiner als 60°, die aus dieser Quelle zu befürchtenden Fehler auf wenige Hundertel der Sekunde herabsinken. Für Sternwarten aber, die über 35° vom Equator entfernt sind, wird kaum je Veranlassung bestehen, bei der Beobachtung der Höhenunterschiede über 60° Z.D. hinauszugehen.

Für die Declinationen, die nach (*c*) bestimmt werden, ist diese Fehlerquelle noch viel weniger zu fürchten. Weiter unten nämlich wird gezeigt, dass der Factor, mit

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* E. F. v. d. Sande Bakhuyzen: Bepaling van de Helling der Ecliptica p. 60. Werden die dort gegebenen Unterschiede für die Correction der Strahlenbrechung verbessert, sowie diese aus den Circumpolarsternen p. 56 gefunden wurde, so kommt nach Abzug der constanten Unterschiede Leidsn – Cap = + 0″·36, Melbourne – Cap = + 0″·68:

Z.D. Leiden.	Cap – Leiden.	Sterne.	Melb. – Leiden.	Sterne.
46°·3	– 0″·03	9	– 0″·24	8
51·7	– 0·27	14	– 0·07	13
57·4	+ 0·30	5	+ 0·10	5
60·8	+ 0·13	8	+ 0·08	7
66·0	+ 0·14	6	+ 0·19	5
69·5	– 0·04	7	– 0·15	6
73·7	– 0·52	5	– 0·26	5
77·2	+ 0·12	6	+ 0·20	4
81·6	0·00	4	+ 0·21	4
86·3	+ 0·47	1	+ 1·02	1

welchem multiplicirt ein *constanter* Fehler in den Beobachtungen der Höhenunterschiede auf die Declinationen der Sterne des südlichen Gürtels, für dessen Mitte $\delta = 2\phi - 90°$ ist, übergeht, immer klein gehalten werden kann. Nur in seltenen Fällen der Praxis ist man gezwungen, den Werth 0·5 ein wenig zu überschreiten, in vielen Fällen aber wird man diesen Werth bis auf 0·3 oder noch weniger herabbringen können. Vorzüglich für die niedrigeren Polhöhen, gerade da, wo man gezwungen wird in grösseren Zenithdistanzen zu beobachten, verkleinert sich dieser Factor in hohem Maasse.

Untersuchungen der zweiten Art sind von Prof. H. G. v. d. Sande Bakhuyzen für Greenwich und Königsberg (Astr. Nach. Bd. 72, p. 241), später von Dr. E. F. v. d. Sande Bakhuyzen für Leiden angestellt (Bepaling etc. p. 39). An die Realität eines Unterschiedes zwischen den beobachteten Zenithdistanzen bei positiven und negativen Werthen der Grösse : Temp. im Beob. Saale minus Temp. ins Freie, ist kaum zu zweifeln; besonders ist bei den zahlreichen Greenwicher Polarisbeobachtungen der Jahre 1851–64 der Einfluss eines solchen Unterschiedes unverkennbar. Aber auch für eine Zahl von südlichen Sternen wurden dergleiche Anomalien gefunden. Für unseren Zweck genügt es einige Zahlen abzuschreiben, die für fast gleiche Entfernungen nördlich und südlich vom Zenith gelten. Zur Vergrösserung des Gewicht zog ich die Resultate für Sterne, die in nicht sehr verschiedenen Zenithdistanzen culminiren, in einem Resultat zusammen. Im folgenden Uebersicht ist z die Z.D. aus Beobachtungen, bei welchen die Temperatur der inneren Luft höher war als die der äusseren, und z' die Z.D. aus Beobachtungen, bei welchen die äussere Temperatur die höhere war, Δt und $\Delta t'$ bezeichnen für beide Fällen die Differenz, innere − äussere Temperatur.

Greenwich.

	Mittl. Z.D.	$z - z'$	$\Delta t - \Delta t'$	Gewicht.	w.F.
Polaris O. C. u. U. C.	38°·5 n.	+ 0″·52	+ 6°·1 Fahr.	385	0″·033
β Tauri — Regulus	39·4 s.	+ 0·21	+ 5·8	136	0·055

Königsberg.

	Mittl. Z.D.	$z - z'$	$\Delta t - \Delta t'$	Gewicht.
Polaris O. C. u. U. C.	34°·8 n.	+ 0″·19	+ 8°·8 Fahr.	18·4
β Tauri — α Ophiuchi	33·4 s.	+ 0·29	+ 8·9	26·9

Leiden.

57 n.	+ 0·023	+ 1°·0 Cels.
11 n.	+ 0·083	+ 1·0
11 s.	− 0·026	+ 1·0
57 s.	+ 0·048	+ 1·0

Die w. F. für die Greenwicher Resultaten wurden erhalten aus der Vergleichung der Werthe der Unterschiede $z - z'$ (alle auf den gleichen Werth von $\Delta t - \Delta t'$ reducirt), welche für die 14 Jahre 1851–64 und für die Obere und Untere Culmination von Polaris in der Abhandlung des Herrn Bakhuyzen p. 243 besonders gegeben werden. Für die südlichen Greenwicher Sterne wurde dann der nämliche w. F. für die Gewichtseinheit angenommen. Die Königsberger und die Leidener Beobachtungen in 57° Z.D. ergeben für den Unterschied $N - S$ eine ganz unerhebliche Quantität, die gewiss nicht verbürgt werden kann. Für die Beurtheilung der Genauigkeit des Leidener Resultats für 11° Z.D. fehlen die Daten; gerade die für die Z.D. 10° N. beobachtete Grösse ist sehr abweichend; würde man darauf keine Rücksicht nehmen, so würde der Unterschied $N - S$ von 0·109 auf 0·082 gebracht, eine Grösse, die kleiner ist als die analoge Grösse für Greenwich. Diese letztere, nämlich $N - S = + 0''\cdot29 \pm 0''\cdot064$, für $\Delta t - \Delta t' = + 6^\circ\cdot0$ Fahr., übertrifft seinen w. F. mehr als 4½ Mal, so dass an ihrer Realität wohl kaum zu zweifeln ist. Dennoch folgt daraus noch nicht, dass man den nämlichen Unterschied finden wird bei directen Messungen eines Höhenunterschiedes; denn der Werth $+ 0''\cdot21$, der für $z - z'$ bei einer südlichen Zenithdistanz von $39^\circ\cdot4$ gefunden ist, beruht auf Beobachtungen von Sternen, deren A.R. im Mittel $6^h\cdot5$ beträgt, während sich der analoge Werth für nördliche Zenithdistanzen gänzlich auf Polaris-Beobachtungen gründet, die zu ganz anderen Zeiten angestellt werden. Herr Bakhuyzen schreibt die Refractionsanomalie, von der hier die Rede ist, der abnormen Gestalt der Luftschichten im Beobachtungssaale zu, welche Gestalt nach seinen Untersuchungen zu verschiedenen Zeiten eine ziemlich constante zu sein scheint. Die Abweichungen aber, welche bei der Annahme einer solchen constanten Gestalt übrig bleiben, und die nach dem Verfasser (S. 247) vielleicht zum grössten Theil von Aenderungen in der mittleren Oberfläche der Luftschichten herrühren, sind theilweise so gross und grösser als die Hälfte des gefundenen Unterschiedes von $0''\cdot29$. Es ist somit jedenfalls die Möglichkeit nicht ausgeschlossen, dass sich bei directen Beobachtungen von Höhenunterschieden der Einfluss einer Temperaturungleichheit der inneren und äusseren Luft viel weniger fühlbar machen wird, als man nach obigen Resultaten fürchten könnte; weil dieses aber nur vermuthungsweise ausgesprochen werden kann, und auch vielleicht für das in Leiden gefundene Resultat gar nicht zutrifft, wird es gut sein schon jetzt anzugeben, wie gross die Fehler sind, die aus einem Unterschiede von dem gefundenen Betrag in der Polhöhe und in den etwa zu bestimmenden Declinationen entstehen.

Die Beobachtungen der Höhenunterschiede werden schon wegen der zu geringen Helligkeit nie bei Tage angestellt werden. Wie aus der Abhandlung des Herrn Bakhuyzen zu entnehmen ist, variirt aber bei den nächtlichen Beobachtungen (wenigstens in Greenwich) der Unterschied der inneren und äusseren Temperatur in den verschiedenen Monaten nur sehr wenig und hat im Mittel einen Betrag von $3^\circ\cdot6$ Fahr. In Leiden wird nahezu derselbe Werth gefunden. Bei den Beobachtungen der Höhenunterschiede ist also ein *fast constanter* Fehler von $\frac{3\cdot6}{6\cdot0} \times 0''\cdot29$ $= 0''\cdot174$ zu befürchten. Dieser Fehler wird in die Polhöhe mit einem Factor eingehen, der sich in der Praxis leicht zwischen den Grenzen 0·5 und 0·9 einschliessen lässt. In der Polhöhe wäre demnach aus dieser Quelle ein Fehler möglich zwischen $0''\cdot09$ und $0''\cdot16$. Für die Declinationen, die nach c (p. 148) bestimmt werden, gilt das oben von einem constanten Fehler in den Höhenmessungen gesagte. Die beiden Anomalien der Refraction werden also nicht verhindern, dass die Declinationen eines Gürtels von südlichen Sternen, für dessen Mitte $\delta = 2\,\phi - 90°$, mit einem Fehler bestimmt werden, der unter $0''\cdot1$ bleibt und in vielen Fällen beträchtlich kleiner sein wird.

Es scheint aber a priori kein sehr schwieriges Problem die Ursache der letzteren Anomalie für die Bestimmung von Höhenunterschieden wenigstens grösstentheils aufzuheben. Denn dazu ist nur eine *symmetrische* Gestalt der mittleren Oberfläche der Luftschichten erforderlich, welche sich jedenfalls viel leichter herstellen lassen wird, als die völlig normale Gestalt, die für eine fehlerfreie Bestimmung der absoluten Zenithdistanzen unentbehrlich wäre. Eine solche Symmetrie muss in der Sternwarte zu Pulkowa, wenigstens mit grosser Annäherung, bestehen. Ich schliesse dieses aus dem folgenden : die Unterschiede, die an dieser Sternwarte zwischen den aus Tag und Nachtbeobachtungen abgeleiteten Zenithdistanzen bestehen (Observations de Poulkova, T. V., Introd. p. (12)), sind, wie schon oben hervorgehoben wurde, mit den soeben behandelten Unterschieden von ähnlicher Natur. Diese Unterschiede aber, welche zum Theil sehr beträchtlich und den Refractionen sehr nahe proportional sind, sind für nördliche und südliche Sterne, die kurze Zeit nach einander culminiren, sehr nahe gleich, wie dies deutlich aus der (l. c.) gegebenen Tabelle hervorgeht. An dem Verticalkreise zu Pulkowa werden daher die Beobachtungen von Höhenunterschieden von dem Einflusse der Tageszeit resp. von einem Unterschied zwischen der inneren und äusseren Temperatur in hohem Grade unabhängig sein, und es dürfte das nämliche Resultat, sobald diesem Punkt besondere Aufmerksamkeit geschenkt würde, wohl auch für andere Sternwarten nicht unerreichbar sein.

Das hier angeführte giebt, wie ich glaube, Grund zu der Hoffnung, dass den hier behandelten Fehlerquellen, bei gehöriger Umsicht, in den meisten Fällen keine Fehler entspringen werden, die über einige wenigen Hundertel der Secunde hinausgehen. Jedenfalls ist man zu dem Schlusse berechtigt, dass, wie gross die aus dieser Quelle hervorgehende Unsicherheit auch sein mag, dieselbe doch gewiss nur ein kleiner Bruchtheil der Unsicherheiten ist, die den totalen Refractionen noch anhaften. Ausser diesen, welche man schwerlich je gänzlich wird vermeiden können, sind mir keine Ursachen bekannt, wodurch systematische Fehler eingeführt werden könnten, die sich nicht leicht eliminiren liessen. Von dieser grossen Unabhängigkeit von systematischen Fehlerquellen, die der symmetrischen Anordnung der Beobachtungen zuzuschreiben ist, ist es leicht sich zu überzeugen. Eine kurze Uebersicht über einige der hauptsächlichsten Fehlerquellen wird dies hoffentlich noch näher beleuchten. Zuerst für die Beobachtungen der Höhenunterschiede.

Von der Biegung des Rohres und des Kreises ist man hier fast ganz unabhängig; denn werden die Beobachtungen mit einem Verticalkreise in der Weise angestellt, dass die Einstellungen der beiden Sterne bei Kreisablesungen, die um 180° verschieden sind, stattfinden, so hat man offenbar nur die *Aenderung* der Biegung über das kleine Intervall, das dem zu messenden kleinen Winkel gleich ist, zu berücksichtigen. Aber auch bei Beobachtungen am Meridiankreise verschwindet die Biegung (welchem Gesetz sie auch befolgen mag) aus der Verbindung der beiden Kreislagen. Wie sich später herausstellen wird, werden die kleinen residuellen Biegungsunterschiede entweder sich gegenseitig fast vollkommen aufheben, oder sie können durch eine Modification der Methode, die am Ende dieses Aufsatzes besprochen wird, vollständig vermieden werden.

Die Theilungsfehler werden vermieden, wenn man nach der Talcott'schen Methode beobachtet (was durch die nämliche Modification der Methode möglich wird). Doch auch bei Kreisablesungen kann man diese unschädlich machen, denn indem, wenigstens bei Beobachtungen am Verticalkreise, die Biegung auf jede Beobachtung ohne Einfluss ist, steht nichts einer wiederholten Versetzung des Kreises im Wege, wodurch man im Stande ist jedes Resultat auf einer beliebigen Zahl von Theilstrichen beziehen zu lassen.

Atmosphärische Dispersion ist offenbar ganz unschädlich. Nur wäre es möglich, dass der Effect der Dispersion auf die Pointirung von helleren und schwächeren Sternen verschieden ist. Es erscheint deshalb rathsam die nördlichen und südlichen Sterne im Mittel von gleicher Grösse zu wählen.

Was nun die Unterschiede in Azimuth betrifft, so können die Aenderungen des Instruments und die Unregelmässigkeiten in dem Uhrgang fast ganz ausser Betracht bleiben, denn die ganze Beobachtung dauert nur wenige Minuten, und überdies werden die Beabachtungen bei zweckmässiger Anordnung in verschiedener Richtung von den Instrumentalfehlern beeinflusst.

Von seitlichen Biegungen und Fehlern der Zapfen ist man ganz frei, weil die Einstellung der beiden Sterne bei ganz gleicher Zenithdistanz Statt findet. Auch der Einfluss der Zapfenfehler auf die Nivellirungen fällt ausser Betracht, weil man nur Niveauunterschiede in Rechnung zu ziehen hat.

Theilungsfehler sind wieder durch wiederholtes Versetzen des Kreises zu eliminiren, sogar die Schraubenfehler kann man bei diesen Messungen (wenn wenigstens die Excentricität des horizontalen Kreises nicht zu gross ist) umgehen. Man darf sich nämlich ohne Bedenken bei den Azimutheinstellungen ein Paar Minuten Spielung erlauben und diese dazu benutzen die Nullpunkte der Microscope bei den Einstellungen beider Sterne in dieselben Entfernungen von einem Theilstriche zu bringen.

Auch von der persönlichen Gleichung macht man sich sehr leicht vollständig unabhängig, denn bei den gemachten Voraussetzungen werden die beiden Sterne bei der Beobachtung nahe symmetrisch zum Meridian stehen. Die Geschwindigkeit ihrer Bewegung ist wegen der sehr nahe gleichen Declination dieselbe, und auch die Neigung dieser Bewegung zur Verticalen wird nur unerheblich verschieden sein. Nur wird der östliche Stern sich nach unten bewegen, während der westliche aufzusteigen scheint, doch kann man diese Ungleichförmigkeit leicht mittelst eines Prismas beseitigen. Ein etwaiger, durch ungleiche Helligkeit der Sterne hervorgerufener Unterschied in der Schätzung der Durchgänge hat keinen Einfluss auf die Polhöhenbestimmung.

Bei einer Bewegung des Instruments entweder in Z.D. oder in Azimuth werden, in Folge der Reibung, Torsionen entstehen. Diese werden in verschiedener Richtung auftreten je nach der Richtung der letzten Bewegung. Ein systematischer Fehler aus dieser Quelle wird vermieden, wenn man immer die letzte Bewegung in derselben Richtung macht.

II. *Ableitung der Formeln.*

Einfachste Form der Methode. In der Praxis wird zwar diese sehr einfache Form wohl niemals in Anwendung kommen, es wird dadurch aber sofort eine Einsicht in die ganze Sache gewonnen. Es seien Σ ein Circumpolarstern und S_1, S_2 zwei südliche Sterne, deren Declinationen innerhalb weniger Secunden* gleich und weiter so gewählt sind, dass S_1 bis auf wenigen Minuten gleichzeitig im Meridian ist mit Σ in O.C., S_2 bis auf wenigen Minuten gleichzeitig mit Σ in U.C. Durch vier Beobachtungen zu verschiedenen Zeiten angestellt bestimme man die folgenden Grössen :

(*a*) In Augenblicken in welchen S_1 und S_2, die erstere östlich, die andere westlich vom Meridian, innerhalb weniger Secunden gleiche Zenithdistanzen, also auch gleiche Azimuthe erreichen—

$$\text{Azimuth } S_2 - \text{Azimuth } S_1 = h$$
$$\text{Beob. Zeit } S_2 - \text{Beob. Zeit } S_1 = \lambda$$

(*b*) In Azimuthen, die innerhalb weniger Secunden mit denjenigen der Beobachtung (*a*) identisch sind, aber jetzt S_2 östlich, S_1 westlich vom Meridian—

$$\text{Azimuth } S_1 - \text{Azimuth } S_2 \quad h'$$
$$\text{Beob. Zeit } S_1 - \text{Beob. Zeit } S_2 = \lambda'$$

h und h^1 offenbar auch nur um wenigen Secunden verschieden.

$$(c) \qquad \text{Zen. Dist. } \Sigma, \text{ O. C.,} - \text{Zen. Dist. } S_1 \text{ im Mer.} = \beta$$
$$(d) \qquad \text{Zen. Dist. } \Sigma, \text{ U. C.,} - \text{Zen. Dist. } S_2 \text{ im Mer.} = \beta'$$

Es wird dann die Polhöhe ϕ bestimmt durch die Gleichung †—

$$(1) \qquad \sin\phi \sin \tfrac{1}{4}(\lambda + \lambda') - \cos\phi \cot\left[2\phi + \tfrac{1}{2}(\beta + \beta')\right] + \cos \tfrac{1}{4}(\lambda + \lambda') \cot \tfrac{1}{4}(h + h') = 0$$

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* Wo hier von " wenigen Secunden " die Rede ist, sind Grössen gemeint, deren Quadraten vernachlässigt werden dürfen.

† Diese Gleichung geht durch sofort ersichtliche Substitutionen aus der bekannten Formel $- \sin\phi \cos t + \cos\phi \operatorname{tang} \delta + \sin t \cot A = 0$ hervor.

woraus ϕ durch successive Annäherung oder auf andere Weise zu berechnen ist. Die Polhöhe wird also unabhängig von den Declinationen und absoluten Azimuthen sein, und nur abhängig von den kleinen Zeitintervallen λ, λ', von den Azimuth Unterschieden h, h' (der Ort des Meridians braucht nur bis auf ein Paar Zehntel der Minute bekannt zu sein) und den Höhenunterschieden β, β'. Diese können klein gemacht werden (wenige Graden), wenn die südlichen Sterne S_1 und S_2 derart gewählt werden, dass bei ihrer Culmination ihre Z.D. nahe der Z.D. des Pols gleich, und weiter Σ in möglichster Nähe des Pols gewählt wird. Die an und für sich schon kleinen Ueberreste von Biegung und Strahlenbrechung, die den β, β' noch anhaften, werden einander in diesem Fall in der Grösse $\beta + \beta'$ noch fast vollständig aufheben.

Will man dieser Methode der Polhöhenbestimmung eine allgemein brauchbare Form geben, so hat man derselben nach zwei verschiedenen Richtungen eine nothwendige Ausdehnung zu geben. Erstens gestattet die Sternfülle des Himmels nur in vereinzelten Fällen Sterne aufzufinden, deren Declinationen bis auf wenige Secunden gleich, deren R.A. aber bis auf ein Paar Minuten um 12^h verschieden sind. Zwar ist letzteres nicht durchaus nothwendig, die Bedingung aber, dass alle extrameridionale Beobachtungen in gleicher Entfernung vom Meridian anzustellen sind, ist Ursache, dass wenn die beiden Unterschiede in R.A. sehr verschieden sind, auch die λ sehr verschieden ausfallen müssen, wovon ein beträchtlicher Zeitverlust die Folge ist. Man hat daher vor Allem die Grenzen der Declinationen und der Azimuthe zu erweitern. Sobald man dies aber thut, indem an der Bedingung festgehalten wird, dass bei den zwei zusammengehörigen Azimuth-Einstellungen die Z.D. gleich sein sollen, ist es nicht mehr möglich einen Werth von ϕ abzuleiten, der von der Declination des Circumpolarsterns und von den absoluten Azimuthen gänzlich unabhängig ist. Dennoch wird man offenbar, so lange die Unterschiede der Declinationen und der absoluten Azimuthe innerhalb gewisser Grenzen gehalten werden, mit leicht zu erhaltenden genäherten Werthen vollständig auskommen. Es kommt darauf an zu zeigen, dass diese Grenzen ohne Bedenken so weit aus einander gerückt werden können, dass dadurch eine völlig genügende Auswahl von Beobachtungs-Objecten gewonnen wird, die es immer ermöglicht ohne Zeitverlust zu arbeiten.

Zweitens wird die Beobachtung, wenn man die Zeitintervallen λ klein zu erhalten wünscht, unmöglich für Polhöhen gleich oder unter 45°, und auch mehrere Grade nördlicher sind die Beobachtungen in viel zu kleinen Höhen anzustellen. Man entgeht dieser Schwierigkeit durch eine Vorgrösserung der Zahl der Sterne.

Beide Erweiterungen sind in der folgenden Modification der oben angegebenen Methode enthalten. Es wird dabei vorausgesetzt: die Beobachtung von n Circumpolarsternen, deren $2n$ Culminationen bis auf wenige Zeitminuten regelmässig über den 24 Stunden vertheilt liegen, und die der Zeit nach nahe mit den oberen Culminationen von $2n$ südlichen Sternen zusammenfallen. Diese letzteren sind so gewählt, dass ihre Zenithdistanzen im Meridian innerhalb einer näher zu bestimmenden Zahl von Graden der Colatitude gleich sind. Die R.A. Unterschiede je zweier auf einander folgenden südlichen Sterne sind bei den gemachten Annahmen einander nothwendiger Weise bis auf wenige Zeitminuten gleich.

Die zufälligen Fahler und ihr Einfluss auf die Polhöhenbestimmung werden dann untersucht; danach werden einige Betrachtungen angestellt über die für verschiedene Polhöhen passende Zahl der Circumpolarsterne. Schliesslich soll eine Modification der Methode besprochen werden, die durch die beschränkte Zahl der nur wenige Grade vom Pol entfernten Sterne wünschenswerth gemacht wird.

Allgemeine Entwickelung. Es seien S_1, S_2, . . . S_{2n} $2n$ südliche Sterne nach der R.A. geordnet, deren Culminationen einander bis auf wenige Minuten nach Intervallen von $\frac{24^h}{2n}$ folgen, und deren Declinationen jedenfalls nur um wenige Grade verschieden sind (es ist zweckmässig die Mitte dieses Gürtels von Sternen nahe auf $\delta = 2\phi - 90°$ fallen zu lassen). Es seien ferner Σ_1, Σ_2 . . . Σ_{2n} n Circumpolarsterne bei ihren *beiden* Culminationen, gleichfalls nach den Durchgangszeiten geordnet und so gewählt, dass diese mit den Culminationen der Sterne S_1 . . . S_{2n} nahe zusammenfallen. Der Stern Σ_k ist demnach identisch mit Σ_{k+n} resp. Σ_{k-n}, nur ist, wenn die erste Bezeichnung sich auf die obere Culmination bezieht, unter der letzteren die untere Culmination zu verstehen, und umgekehrt.

In dem Augenblicke des Durchganges von jedem Paar Sterne S_k, Σ_k, werde nun der Unterschied der Z.D. beobachtet. Weiter bestimme man den Unterschied der Azimuthe und den entsprechenden Unterschied der Beobachtungszeiten von jedem Paar Sterne S_k, S_{k+l} (deren R.A. Unterschied $= l \cdot \frac{24^h}{2n}$, wo l eine ganze, positive Zahl $< 2n$ ist) in Augenblicken, in welchen ihre Z.D. gleich sind. Ich behaupte dann, dass sobald man eine vollständige Reihe dieser Beobachtungen erhalten hat, d. h., sobald alle $2n$ Höhenunterschiede bestimmt sind, und man $2n$ Unterschiede in Azimuth beobachtet hat, wobei jeder Stern S einmal östlich und einmal westlich vom Meridian eingestellt ist, man alles zur Polhöhenbestimmung erforderliche gewonnen hat.

Es seien k eine der Zahlen 1, 2, . . . $2n$, α_k und δ_k die R.A. und Decl. des Sternes S_k, Δ_k die Decl. des Sternes Σ_k; diese wird für die U.C. über 90° hinaus gezählt, das k wird daher auch hier von 1 bis $2n$ gezählt. Offenbar ist immer—

$$\Delta_k + \Delta_{k \pm n} = 180° \tag{2}$$

Ferner seien A_{kw}, T_{kw}, t_{kw}, z_{kw}, ω_{kw} das absolute Azimuth, die Durchgangszeit, der Stundenwinkel, die Z.D., der parallactische Winkel des Sternes S_k bei seiner Beobachtung westlich vom Meridian, und A_{ko}, T_{ko} . . . dieselben Grössen östlich vom Meridian; β_k die im Meridian beobachtete Grösse: Z.D. Σ_k − Z.D. S_k; λ, h beobachtete Grössen bestimmt durch die Gleichungen—

$$\lambda_{k,k+l} = T_{kw} - T_{k+l\,o} \tag{3}$$

$$h_{k,k+l} = A_{kw} - A_{k+l\,o} \tag{4}$$

Mit dieser Bezeichnung ergeben die $6n$ beobachteten Grössen λ, h, β die $6n$ Gleichungen—

$$a_k - a_{k+1} + t_{kw} - t_{k+w} = \lambda_{k,k+1} \tag{5}$$
$$A_{kw} - A_{k+w} = h_{k,k+1} \tag{6}$$
$$\delta_k + \Delta_k - 2\phi = \beta_k \tag{7}$$

Weiter bestehen zwischen den hier eingeführten Unbekannten noch diese $4n$ Gleichungen—

$$\left\{\begin{array}{l} -\sin\phi\cos t_{kw} + \cos\phi \operatorname{tg}\delta_k + \sin t_{kw} \cot A_{kw} = 0 \\ -\sin\phi\cos t_{ko} + \cos\phi \operatorname{tg}\delta_k + \sin t_{ko} \cot A_{ko} = 0 \end{array}\right. \tag{8}$$

Die Zahl der unbekannten Grössen ist also, wenn die jedesmalige Meridianablesung, somit die absoluten Azimuthe als Unbekannten angesehen werden, $13n + 1$; die Zahl der Gleichungen zu ihrer Bestimmung dagegen nur $10n$. Dennoch kann, bei einer Beobachtungsweise wie die hier vorausgesetzte, die Unbekannte ϕ unter gewissen Bedingungen praktisch ganz unabhängig von den übrigen Unbekannten aufgelöst werden. Um dieses zu zeigen nehme ich für alle unbekannten Grössen Näherungen an, welche durch ein angehängtes ° unterschieden werden sollen, so dass z. B. $\Delta_k = \Delta^\circ_k + d\Delta_k$ u.s.w. Die Näherungen für die Elemente des Dreiecks Pol, Zenith, Stern werden mit einander vereinbar angenommen. Man hat zu diesem Zweck, sobald z. B. einmal für die ϕ, δ_k, A_k Näherungen festgestellt sind, die t°_k zu berechnen nach—

$$-\sin\phi^\circ \cos t^\circ_{kw} + \cos\phi^\circ \operatorname{tg}\delta^\circ + \sin t^\circ_{kw} \cot A^\circ_{kw} = 0 \tag{9}$$

und der analogen Gleichung für t°_{ko} u.s.w. Man setze ferner—

$$a^\circ_k - a^\circ_{k+1} + t^\circ_{kw} - t^\circ_{k+w} = \lambda^\circ_{k,k+1} \tag{10}$$
$$A^\circ_{k,w} - A^\circ_{k+w} = h^\circ_{k,k+1} \tag{11}$$
$$\delta^\circ_k + \Delta^\circ_k - 2\phi^\circ = \beta^\circ_k \tag{12}$$

Offenbar sind jetzt die Differentiale die wirklichen Unbekannten, zu deren Bestimmung die folgenden Gleichungen verwendbar sind—

$$da_k - da_{k+1} + dt_{kw} - dt_{k+w} = \lambda_{k,k+1} - \lambda^\circ_{k,k+1} \tag{13}$$
$$dA_w - dA_{k+w} = h_{k,k+1} - h^\circ_{k,k+1} \tag{14}$$
$$d\delta_k + d\Delta_k - 2d\phi = \beta_k - \beta^\circ_k \tag{15}$$

und weiter, wenn zur Abkürzung die Elemente z und ω eingeführt werden, nach einer bekannten Differentialformel—

$$dt_{kw} = \frac{\sin z^\circ_{kw}}{\cos\omega^\circ_{kw} \cos\delta^\circ_k}\, dA_{kw} + \frac{\cos z^\circ_{kw} \sin A^\circ_{kw}}{\cos\omega^\circ_{kw} \cos\delta^\circ_k}\, d\phi - \frac{\operatorname{tg}\omega^\circ_{kw}}{\cos\delta^\circ_k}\, d\delta_k \tag{16}$$

und eine analoge Gleichung fur dt_{ko}. Bei der Auflösung dieser Gleichungen ist zu bemerken, dass, wiewohl in den $2n$ Gleichungen (13) $2n$ unbekannte Rectascensionen vorkommen, sich dennoch immer eine Gleichung daraus ergiebt, aus der dieselben alle eliminirt sind, und zwar ist einleuchtend, dass man dazu nur die Summe sämmtlicher Gleichungen (13) zu nehmen hat. Im Allgemeinen aber werden mehr als eine Gleichung dieser Art gebildet werden können, denn sobald die Reihe $\alpha_k, \alpha_{k+l} \dots \alpha_{k+2l} \dots$ sich schliesst, wird offenbar die Summe der Differenzen $\alpha_{k+} - \alpha_1, \alpha_{k+l} - \alpha_{k+l} \dots$ verschwinden. Die Zahl dieser von den R.A. unabhängigen Gleichungen wird immer dem grössten gemeinschaftlichen Theiler von $2n$ und l gleich sein (die Einheit und die Zahl l selbst auch als Theiler mitgerechnet).

Zur Polhöhenbestimmung kommen nur diese Gleichungen zur Verwendung. Folglich kommen dabei nicht die Grössen λ selbst in Betracht, sondern nur die $\Sigma\lambda_{k,k+l}$ oder mit anderen Worten nur Grössen wie $\Sigma\,(T_{kw} - T_{k+l,o})$ wo die Summe sich über eine geschlossene Reihe von Sternen erstreckt, sodass wenn die östliche Durchgangszeit eines Sterns darin erscheint, auch die westliche Durchgangszeit desselben Sterns vorkommt, und die Summe daher in einer Reihe von Gliedern von der Form $T_{kw} - T_{ko}$ aufgelöst werden kann. Hieraus folgt dass jeder Fehler, der in T_{ko} und T_{kw} d.h. in der östlichen und westlichen Durchgangszeit desselben Sterns den gleichen Betrag hat, aus dem Resultat verschwindet.

Vorläufig werde ich, der Allgemeinheit wegen, nur die Summe aller Gleichungen (13) benutzen. Statt dieser $2n$ Gleichungen hat man somit nur diese einzige.*

$$\Sigma dt_{kw} - \Sigma dt_{ko} = \Sigma(\lambda - \lambda_o) \tag{17}$$

Zur Elimination der δ werden die Gleichungen (15) in (16) gebracht und dabei zur Abkürzung gesetzt—

$$C = \frac{\sin z^\circ}{\cos\omega^\circ \cos\delta^\circ} = \frac{\sin t^\circ}{\sin A^\circ \cos\omega^\circ} \tag{18}$$

$$E = \frac{\operatorname{tg}\omega^\circ}{\cos\delta^\circ} \tag{19}$$

$$D = 2\frac{\operatorname{tg}\omega^\circ}{\cos\delta^\circ} - \frac{\cos z^\circ \sin A^\circ}{\cos\omega^\circ \cos\delta^\circ} = 2E\left(1 - \frac{\cos z^\circ \cos\delta^\circ}{2\cos\phi^\circ}\right) \tag{20}$$

Man erhält—

$$\left\{\begin{aligned} dt_{kw} &= C_{kw}dA_{kw} - D_{w}d\phi - E_{kw}\,(\beta_k - \beta^\circ_k) + E_{kw}d\Delta_k \\ dt_{ko} &= C_{ko}dA_{ko} - D_{ko}d\phi - E_{ko}\,(\beta_k - \beta^\circ_k) + E_{ko}d\Delta_k \end{aligned}\right. \tag{21}$$

Werden jetzt diese in (17) eingesetzt, so ergiebt sich—

$$\begin{aligned} d\phi\Sigma\,(D_{kw} - D_{ko}) = {} & \Sigma\,(\lambda - \lambda^\circ) - \Sigma\,(F_{kw} - E_{ko})\,(\beta_k - \beta^\circ_k) \\ & + \Sigma\,(E_{kw} - E_{ko})\,d\Delta_k + \Sigma C_{kw}dA_{kw} - \Sigma C_{ko}dA_{ko} \end{aligned}$$

und schliesslich, wenn man in den zwei letzten Gliedern die dA vereinigt, welche sich auf dieselbe Beobachtung beziehen, und die Gleichungen (14) einsetzt—

$$\begin{aligned} d\phi\Sigma\,(D_{kw} - D_{ko}) = {} & -\Sigma\,(\lambda - \lambda^\circ) + \tfrac{1}{2}\Sigma\,(C_{kw} + C_{k+lo})\,(h_{k,k+l} - h^\circ_{k,k+l}) \\ & - \Sigma\,(E_{kw} - E_{ko})\,(\beta_k - \beta^\circ_k) + \Sigma\,(E_{kw} - E_{ko})\,d\Delta_k \\ & + \tfrac{1}{2}\Sigma\,(C_{kw} - C_{k+l,o})\,(dA_{kw} + dA_{k+l,o}). \end{aligned} \tag{22}$$

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* Für $n = 1$ hat man nur zwei Gleichungen von der Form (13). Die Differenz dieser wird eine Bestimmung der Differenz $\alpha_1 - \alpha_2$ abgeben, welche von den nämlichen Fehlern frei ist, wovon man sich bei der Polhöhenbestimmung unabhängig macht, nur nicht von dem von der Helligkeit abhängigen persönlichen Fehler. Im Allgemeinen hat man n Bedingungsgleichungen zwischen den $2n - 1$ R.A. Differenzen.

Man zieht aus dieser Gleichung sofort den Schluss, zu welchem wir schon früher für den Fall $n = 1$ gelangt waren, dass wenn alle δ innerhalb weniger Secunden gleich sind, und man weiter die Azimuth-Beobachtungen alle in Entfernungen vom Meridian anstellt, die innerhalb nur weniger Secunden variiren, die Polhöhe sich unabhängig von den Δ_k und den absoluten Azimuthen bestimmen lässt, denn in diesem Falle kann man für alle δ_k die nämliche Näherung δ annehmen. Ebenso kann allgemein gesetzt werden: $A^\circ_{kw} = A$ und $A^\circ_{ko} = 360^\circ - A$, und ferner, weil offenbar auch die Stundenwinkel bei den Azimuth-Einstellungen nahe gleich werden: $t^\circ_{kw} = t$ und $t^\circ_{ko} = 360^\circ - t$. Mit diesen Werthen werden nun nach (18)–(20) auch alle C identisch, und das gleiche gilt von dem absoluten Werthe aller E und D, sodass sich aus (22), wenn die Zeiger weggelassen werden, ergiebt—

$$(23) \qquad n d\phi = -\frac{1}{4D}\,\Sigma(\lambda - \lambda^\circ) + \frac{C}{4D}\,\Sigma(h - h^\circ) - \frac{E}{2D}\,\Sigma(\beta - \beta^\circ)$$

aus welcher die zwei letzten Glieder herausgefallen und worin C, D, E, nach (18)–(20) zu rechnen sind. Dass wirklich das vorletzte Glied verschwindet ist klar, wenn man bedenkt, dass wenn $\Delta_{k \pm n}$ einer unteren Culmination angehört, dadurch die Declination über den Pol hinaus gezählt bezeichnet wird, sodass nach (2) $d\Delta_k = -d\Delta_{k\pm n}$, woraus hervorgeht, dass in der Summe $\Sigma\,(E_{kw} - E_{ko})\,d\Delta_k$, die hier zu $2\Sigma E d\Delta_k$ wird, immer die zwei Glieder, die sich auf demselben Stern beziehen, von entgegengesetztem Vorzeichen sind.

Wie schon früher bemerkt, lässt sich eine fast vollständige Gleichheit der Declinationen nur ausnahmsweise erreichen, und man wird nur mit Aufopferung vieler Zeit alle Beobachtungen im nämlichen Azimuthe (pos. oder neg.) anstellen können. Es ist aber leicht zu zeigen, dass auch zwischen genügend weite Grenzen, bei der Annäherung mit welcher die Δ und die absoluten Azimuthe in der Regel bekannt sind, die zwei letzten Glieder von (22) verschwinden.

Für Polhöhen zwischen $+70^\circ$ und $+20^\circ$ oder zwischen -70° und -20° hat man die Declinationen der Sterne S innerhalb oder nur wenig ausserhalb der Grenzen $\delta = +50^\circ$ und $\delta = -50^\circ$ zu wählen. Nach der Bonner Durchmusterung kommt zwischen 0° und + 50° im Mittel ein Stern 1·0 bis 8·0 mag. auf jedes Rechteck, dessen Seiten 1° und 4^m sind. In einem willkürlichen Reckteck aber, dessen Seiten 1° und 8^m sind, wird nur ausnahmsweise ein solcher Stern fehlen. Fängt man daher bei einem willkürlichen Stern an, dessen Decl. zwischen $2\phi - 89^\circ\,30'$ und $2\phi - 90^\circ\,30'$ liegt, so wird man gewöhnlich dabei eine Reihe von Sternen finden können, die in dem 1° breiten Gürtel liegen, und deren Entfernungen in R.A. von dem gewählten Stern innerhalb $\pm\,4^m$ gleich $\frac{24^h}{2n}$, $2\,\frac{24^h}{2n}$, $3\,\frac{24^h}{2n} \cdots$ sind. Ebenso wird es bei den meisten der 15 Sterne, die im Mittel in einem Theile des genannten Gürtels vorkommen, dessen Länge eine Stunde in R.A. ist, gelingen, eine derartige Reihe zu bilden. Daraus ersieht man, dass man die Beobachtungen jedenfalls einander zehn oder noch weniger Minuten folgen lassen kann. Wird man nicht weiter als bis zür Grösse 7·0 hinabgehen, so ist jedenfalls eine doppelt so grosse Spielung in den beiden Richtungen mehr als genügend.

Um jetzt die Werthe, welche die zwei letzten Glieder der Gleichung (22) erreichen, zu übersehen, habe ich ihren Ausdruck weiter entwickelt. Grössen wie E_{kw}, C_{kw} sind von dem Orte des Sternes S_k bei seiner westlichen Einstellung abhängig, und dieser selbst wird wieder durch die Bedingung bestimmt, dass die Beobachtung der Sterne S_k und S_{k+1} in gleicher Entfernung vom Zenith stattfinden muss. Dies erreicht man, wenn man in dem folgenden Stundenwinkel einstellt (ich begnüge mich hier mit den ersten Potenzen der Decl. Unterschiede)—

$$(24)\quad \begin{cases} t_{kw} = \frac{1}{2}(a_{k+1} - a_k) + \frac{1}{2}\lambda + \dfrac{\operatorname{tg}\phi - \cos t \operatorname{tg}\delta}{2 \sin t}(\delta_k - \delta_{+1}) \\ t_{k+1,o} = -\frac{1}{2}(a_{k+1} - a_k) - \frac{1}{2}\lambda + \dfrac{\operatorname{tg}\phi - \cos t \operatorname{tg}\delta}{2 \sin t}(\delta_k - \delta_{k+1}) \end{cases}$$

wo für die Grössen in dem Coefficient des letzten Gliedes genäherte mittlere Werthe benutzt werden können.

Unter Einführung des mittleren Werthes—

$$(25)\quad t = \frac{1}{2n}\Sigma t = \frac{l}{n}\,90^\circ + \frac{1}{4n}\Sigma\lambda$$

und wenn man zur Abkürzung—

$$(26)\quad \mu_{,k+1} = \tfrac{1}{2}(a_{k+1} - a_k) - \frac{l}{n}\,90^\circ + \tfrac{1}{2}\lambda - \frac{1}{4n}\Sigma\lambda$$

setzt, erhält man aus (24)—

$$(27)\quad \begin{cases} t_{kw} = t + \mu_{k,k+1} + \dfrac{\operatorname{tg}\phi - \cos t \operatorname{tg}\delta}{2 \sin t}(\delta_k - \delta_{k+1}) = t + \mu_{k,k+1} + \dfrac{\delta_k - \delta_{k+1}}{2E\cos^2\delta} \\ t_{k+1,o} = -t - \mu_{k,k+1} + \dfrac{\operatorname{tg}\phi - \cos t \operatorname{tg}\delta}{2 \sin t}(\delta_k - \delta_{k+1}) = t + \mu_{k,k+1} + \dfrac{\delta_k - \delta_{k+1}}{2E\cos^2\delta} \end{cases}$$

wo die zwei letzten Glieder kleine Grössen sind. Mit diesem Stundenwinkel werden sehr leicht die übrigen Grössen A, w, z, C, E berechnet, deren Abweichungen von einem mittleren Betrag offenbar auch nur kleine Grössen sein werden, die nur von den Grössen μ und den Decl. Unterschieden abhängig sind. Hat man auf diese Weise die Form der E und C entwickelt, so wird man, bevor man die Werthe des ersteren in der vorletzten Summe von (22) einführt, die Glieder dieser Summe noch paarweise vereinigen. Wird nämlich die Gleichung $d\Delta_{k\pm n} = -d\Delta_k$ eingeführt, so erhält dieses Glied die Form—

$$\Sigma\,[E_{kw} - E_{k\pm n,w} - E_{ko} + E_{k\pm n,o}]\,d\Delta_k$$

worin aber jetzt die Summe sich nur über die oberen Culminationen erstreckt; hierin sind die Unterschiede $E_{kw} - E_{k\pm n,w}$ und $E_{k,o} - E_{k\pm n,o}$ offenbar kleine Grössen.

Werden alle angedeuteten Operationen ausgeführt, so findet man für die zwei letzten Glieder der (22), wenn noch gesetzt wird—

$$k' = k \pm n \tag{28}$$

$$\Sigma^{n}_{1}\left[G(\delta_k - \delta_{k'}) - F(\delta_{k+1} + \delta_{k-1} - \delta_{k'+1} - \delta_{k'-1}) + K(\mu_{k,k+1} + \mu_{k-1,k} - \mu_{k',k'+1} - \mu_{k'-1,k'})\right]d\Delta_k \\ + N\Sigma^{2n}_{1}(\delta_k - \delta_{k+1})(dA_{k,w} + dA_{k+1,o}) \tag{29}$$

wo—

$$\left\{\begin{aligned} F &= \tfrac{1}{2}C\,\frac{\cot A}{\sin^2 z} \\ G &= 2F + 2E\left(2\operatorname{tg}\delta + \frac{1}{\operatorname{tg} t \cot\omega \cos\delta}\right) \\ K &= E\,\frac{\cos A}{\cos\omega \sin t} \\ N &= \tfrac{1}{2}C\,\frac{\cot z}{\cos\omega} \end{aligned}\right. \tag{30}$$

Für die Grössen, welche in den Coefficienten vorkommen, wird man genäherte mittlere Werthe mit dem Vorzeichen nehmen können, welches sie bei der westlichen Einstellung haben. Für die wichtigsten Fälle habe ich den Werth dieser Coefficienten berechnet. Diese wurden noch durch $4n$ × den mittleren Werth von D dividirt und so erhalten (angenommen, dass in (29) δ und μ in Graden ausgedrückt sind)—

		G	F	K	N
$\frac{l}{n} = 1$	$\phi = 60°$	0·0040			0·0016
	55	·0021			·0008
	50	·0009			·0003
$\frac{l}{n} = \frac{2}{3}$	55	·0038	0·0003	0·0004	·0008
	50	·0024	·0003	·0005	·0005
	45	·0016	·0003	·0006	·0003
$\frac{l}{n} = \frac{1}{2}$	45	·0048		·0017	·0009
	40	·0035		·0017	·0007
	30	·0020		·0018	·0004
	20	·0011		·0020	·0002

Erlaubt man sich nun für die a eine Spielung von $\pm 4^{m}$, so wird man nach (26), weil man willkürlich über das Vorzeichen von λ verfügen kann (d. h. entweder die westliche oder die östliche Einstellung vorangehen lassen kann), und wenn $\lambda = 8^{m}$, den μ denselben Werth innerhalb $+ 4^{m}$ geben können. Ist λ grösser, so bleibt dieser Spielraum unverändert, dagegen wird er noch kleiner wenn $\lambda < 8^{m}$. Mit den soeben gefundenen Coefficienten findet man demnach, wenn für die a ein Spielraum von $\pm 4^{m}$, für die δ ein Spielraum von $\pm 30'$ gestattet wird, daher alle δ zwischen

$2\phi - 90° \pm 30'$, alle μ zwischen Const. $\pm 1°$ genommen werden, in dem Ausdruck für $d\phi$—

		Max. Coeff. von Δ_k.	Max. Summe aller Δ Coeff.	Max. Coeff. von $dA_{kw} + dA_{k+1,o}$	Max. Summe dieser Coeff.
$\frac{l}{n} = 1$	$\phi = 60°$	0·0040	0·0040	0·0016	0·0000
	55	·0021	·0021	·0008	
	50	·0009	·0009	·0003	
$\frac{l}{n} = \frac{2}{3}$	55	·0060	·0136	·0008	·0000
	50	·0050	·0098	·0005	
	45	·0044	·0076	·0003	
$\frac{l}{n} = \frac{1}{2}$	45	·0116	·0164	·0008	·0000
	40	·0103	·0138	·0006	
	30	·0092	·0112	·0004	
	20	·0089	·0100	·0002	

Bei Grenzen, die in beiden Richtungen zwei Mal weiter sind, hat man diese Werthe zu verdoppeln. Wenn man nur Sterne bis 7·0 mag. beobachtet, gilt ungefähr dasselbe. Aus den zwei letzten Spalten geht hervor, dass in beiden Fällen ein kleiner Fehler in dem angenommenen Orte des Meridians ohne Einfluss bleibt, und dass ein grösserer aber bei der ganzen Reihe constanter Fehler in den absoluten Azimuthen ganz unschädlich ist. Auch die Coefficienten von Δ sind sehr klein, und die mittleren Werthe werden jedenfalls sehr viel kleiner. Der maximal Coefficient, mit welchem ein constanter Fehler in allen angenommenen Declinationen in $d\phi$ eingeht, ist wie man sieht auch klein und wird sehr selten erreicht; durch eine sorgfältige Wahl der Objecte kann man sie fast beliebig verkleinern. Sobald man also Näherungen für die Δ_k besitzt, die sich nicht beträchtlich mehr als 1″ oder 2″ von der Wahrheit entfernen, kann man immer die zwei letzten Glieder von (22) vernachlässigen.

Dabei ist vorausgesetzt, dass man die direct beobachteten Azimuthe als Näherungswerthe für diese Grössen verwendet, so dass die h und h_o identisch werden und (22) wird—

$$d\phi \Sigma (D_{k,w} - D_{k+1,o}) = - \Sigma (\lambda - \lambda^{o}) - \Sigma (E_{kw} - E_{ko}) (\beta_k - \beta^{o}_{k}) \tag{31}$$

welche eine für die Berechnung sehr bequeme Form hat.

Näherungen für die Δ und den Ort des Meridians wie die hier vorausgesetzten sind natürlich leicht zu erhalten, meistens werden die Beobachtungen selbst, ohne besondere Messungen auszuführen, sehr genaue Werthe für die Δ und die Meridianablesung liefern. Wie schon bemerkt, ist nicht zu erwarten, dass man die Beobachtungen zur Polhöhenbestimmung anstellen wird ohne zugleich auch die Beobachtungen der grössten Azimuthe der benutzten Circumpolarsterne anzuschliessen. Werden aber diese an den nämlichen Abenden angestellt, an welchen auch die anderen Azimuthalbeobachtungen gemacht werden, so liefern sie nicht nur die Δ_k (und dadurch nach (15) die δ_k), sondern man erhält dadurch überdies sehr genaue Bestimmungen des Meridianortes. In diesem Fall könnte man sich viel grössere Unterschiede in den δ erlauben, ja so grosse, dass der südliche

Gürtel von Sternen ebenso breit wird wie der, worin die Circumpolarsterne eingeschlossen sind (siehe unten).

Eine wichtige Sache wurde bis jetzt noch nicht erwähnt, nämlich die Grösse der Höhenunterschiede, die man zu messen hat. Diese Grösse ist offenbar von der Distanz abhängig, bis zu welcher man sich vom Pole zu entfernen hat, um eine genügende Zahl von Sternen zu erhalten. Geht man bis zur 8·0 Grösse hinab, so wird man sich wohl auf den Gürtel $\delta = +84°$ bis zum Pol beschränken können, will man aber keine Sterne schwächer als 7·0 mag. benutzen, so wird man diesen Gürtel etwa bis zu $\delta = +81°$ oder 80° ausdehnen müssen. In diesen beiden Fällen werden daher Höhenunterschiede von 6° resp. 10° vorkommen, welche gewiss nicht unbeträchtlich sind. Dennoch wird die Polhöhenbestimmung nur in äusserst geringem Maasse von dem Gesetze der Biegung (diese als continuirlich vorausgesetzt) und der Refraction beeinflusst werden. Denn erstens ist für einem Höhenunterschied von 6° ja selbst von 10° der Effect der Biegung gewiss immer sehr klein, zweitens werden β_k und $\beta_{k \pm n}$ (in welchen der nämliche Polarstern in seinen beiden Culminationen vorkommt) fast den gleichen absoluten Werth aber mit entgegengesetztem Vorzeichen haben, und weil ausserdem alle β in (31) mit nur wenig verschiedenen Coefficienten auftreten, werden sich die Biegungs-Einflüsse paarweise fast völlig aufheben. Befürchtet man Aenderungen der Biegung, so beobachte man an jedem Abend so ziemlich die gleiche Zahl von Polarsternen in beiden Culminationen. Die Fehler der Refractionen sind aus demselben Grunde unschädlich, wenigstens in höheren Breiten. Für die kleineren Polhöhen aber werden diese Fehler nicht mehr ganz fortgeschafft werden können, und es wird hier lieber die unten vorgeschlagene Modification der Methode in Anwendung zu bringen sein, bei welcher man von der Biegung und der Refraction ganz frei wird.

Wir werden nun kurz die Formeln für die Berechnung der Azimuth-Einstellungen und die zur Reduction der Beobachtungen nothwendigen zusammenstellen. Schon wurden (24) für kleine δ Unterschiede die Werthe der Stundenwinkel angegeben, in welchen die Sterne S_k und S_{k+l} mit einer Zwischenzeit λ in gleicher Höhe gelangen, der erste westlich, der zweite östlich vom Meridian. Die Ableitung dieser Formel in ihrer strengen Form lasse ich hier folgen.

Der Stern (α_k, δ_k) wird zur Zeit $T_{k,w}$ westlich vom Meridian in einem Stundenwinkel $t_{k,w} = T_{k,w} - \alpha_k$ beobachtet. Ebenso der Stern $(\alpha_{k+l}, \delta_{k+l})$ zur Zeit $T_{k+l,o}$ im Stundenwinkel $t_{k+l,o}$. Die Z.D. werden durch die Gleichungen bestimmt—

$$\cos z_{k,w} = \sin \delta_k \sin \phi + \cos \delta_k \cos \phi \cos t_{kw}$$
$$\cos z_{k+l,o} = \sin \delta_{k+l} \sin \phi + \cos \delta_{k+l} \cos \phi \cos t_{k+l,o}$$

Erstens soll nun $z_{kw} = z_{k+l,o}$, zweitens soll zwischen der östlichen und westlichen Beobachtung eine im voraus bestimmte zur Veränderung der Einstellung genügende Zeit $\lambda_{k,k+l}$ vergehen. Man erhält dann leicht—

$$(32) \qquad \begin{cases} t_{k,w} &= T + \tau \\ t_{k+l,o} &= -T + \tau \end{cases}$$

wo T und τ durch die Gleichungen bestimmt werden—

$$T = \tfrac{1}{2}(\alpha_{k+i} - \alpha_k) + \tfrac{1}{2}\lambda_{k,k+i} \tag{33}$$

$$\left\{\begin{aligned} \operatorname{tg} \theta &= \frac{\operatorname{tg} \frac{1}{2}(\delta_k + \delta_{k+i})}{\operatorname{tg} T} \operatorname{tg} \tfrac{1}{2}(\delta_k - \delta_{k+i}) \\ \sin(\tau + \theta) &= \frac{\operatorname{tg} \phi \cos \theta}{\sin T} \operatorname{tg} \tfrac{1}{2}(\delta_k - \delta_{k+i}) \end{aligned}\right. \tag{34}$$

welche für kleine Declinationsunterschiede die einfache Form (24) annehmen. Sobald nach diesen Formeln die Stundenwinkel berechnet sind, findet man das Azimuth, die Zenithdistanz und den parallactischen Winkel durch die Gaussischen Formeln—

$$\cos \tfrac{1}{2}z \sin \tfrac{1}{2}(A - \omega) = \sin \tfrac{1}{2}t \sin \tfrac{1}{2}(\phi + \delta) \tag{35}$$

u.s.w.

Wir kommen nun zur Berechnung von $d\phi$ aus den Beobachtungsdaten. Für ϕ, α_k, δ_k, Δ_k werden die Näherungen ϕ°, α°_k, δ°_k, Δ°_k angenommen, so dass für alle Declinationen und R.A. für eine bestimmte Epoche Näherungen angenommen werden (für die Δ_k möglichst genaue), woraus in aller Schärfe die scheinbaren Oerter für die Beobachtungstage berechnet werden. Für die absoluten Azimuthe nehme man als Näherungen $A_\circ$, die direct beobachteten (d. h. mit einer bis auf wenige Secunden genauen Meridianablesung erhaltenen) Azimuthe. Man hat dann nach (31) zu rechnen. Die Coefficienten wie $D_{k,w}$, $E_{k,w}$ u.s.w. welche in dieser Gleichung vorkommen, sind blos von den Grössen ϕ°, δ°_k, $A^\circ_{k,w}$ abhängig, denn die in (19) und (20) auftretenden t°, ω° und z° werden durch diese Grössen bestimmt. Die Coefficienten sind also für eine bestimmte Polhöhe in kleinen Tafeln mit doppeltem Argumente δ und A zu bringen, die nach (19) und (20) zu berechnen sind, nachdem die ω, t, z, auf bekannte Weise aus ϕ, δ, A abgeleitet sind. Drei oder vier Argumente in jeder Richtung werden oft schon ausreichen.

Die β° werden nach der Formel (12) berechnet, und schliesslich die λ° nach (9) und (10). Nur die Berechnung der t nach (9) hat einige Schwierigkeit, in sofern sie siebenstellig zu führen ist. Bequemer wird bekanntlich diese Berechnung nach den Formeln —

$$\left\{\begin{aligned} \operatorname{tg} B &= \sin \phi^\circ \operatorname{tg} A^\circ \\ \sin(B - t^\circ) &= \cot \phi^\circ \operatorname{tg} \delta^\circ \sin B. \end{aligned}\right. \tag{36}$$

Mit den so gefundenen Werthen der t° ergeben sich die λ° aus der Formel (10). Will man keine Rechenfehler von $0''\cdot01$ gestatten, so reichen die Formeln (36) bei siebenstelliger Rechnung nicht immer aus, es würde aber nicht schwer sein, noch schärfere Formeln aufzustellen. Ist das nämliche Paar Sterne mehr als einmal beobachtet, so hat man die siebenstellige Rechnung nur einmal auszuführen ; die Differentialformel—

$$dt = C \cdot dA - E d\delta \tag{37}$$

wird dann bequem die andere t finden lassen. Man kann für die A auch andere Näherungen annehmen als die direct beobachteten Azimuthe, hat aber dann nach (22) zu rechnen, worin die zwei letzten Glieder weggelassen werden. In der Praxis würde dies ganz unnöthig umständlich, im folgenden aber, wo es sich darum handelt, eine Uebersicht über die zufälligen Fehler zu erlangen, ist es erwünscht von dieser Form auszugehen.

III. *Die zufälligen Fehler und ihr Einfluss auf die Polhöhenbestimmung.*

Bei der Betrachtung des Einflusses der zufälligen Fehler wird also die Gleichung (22) zu Grunde gelegt. Die Coefficienten der h_k und die der β_k werden immer so wenig verschieden sein, dass man offenbar nur sehr unerheblich von der Wahrheit abweichende Resultate für die wahrscheinlichen Fehler erhält, wenn man für alle denselben mittleren Werth einführt. Dadurch aber geht (22) bei Vernachlässigung der zwei letzten Glieder, in (23) über. Uebrigens wird angenommen, dass die Declinationen der südlichen Sterne alle gleich $2\phi - 90°$ sind.

Theils zur Bestimmung der Genauigkeit der Polhöhe, theils auch um die Wahl der vortheilhaftesten Werthe von $\frac{l}{n}$ zu erleichtern durch eine Uebersicht über die verschiedenen Umstände der Beobachtungen und der dabei zu erreichenden Genauigkeit, berechnete ich die folgende Tabelle. Darin sind t, z, ω, A der mittlere Stundenwinkel, die Zenithdistanz, der parallactische Winkel und das Azimuth bei den Azimuth-Einstellungen. Daneben sind die numerischen Werthe der Coefficienten der Gleichung (23) angesetzt. Die Z.D. bei den Messungen der Höhenunterschiede brauchte nicht angeführt zu werden, weil sie immer der Colatitude gleich vorausgesetzt wird. In der letzten Spalte sind die wahrscheinlichen Fehler der Polhöhenbestimmung, die wir sofort näher besprechen werden, eingetragen.

	$\frac{l}{n}$	t	z	ω	A	$\frac{1}{4D}$	$\frac{C}{4D}$	$\frac{2E}{4D}$	ρ_ϕ
$\phi = +60°$	$\frac{7}{12}$	52°·5	45°·9	33°·6	73°·3	0·41	0·41	0·63	0″·65
$\delta \quad +30$	$\frac{8}{12}$	60·0	49·5	34·7	80·5	·36	·38	·57	·59
	$\frac{9}{12}$	67·5	53·2	35·2	87·3	·32	·36	·52	·54
	$\frac{10}{12}$	75·0	57·0	35·2	93·8	·29	·34^5	·47	·49
	$\frac{11}{12}$	82·5	60·7	34·6	100·0	·27	·33	·43	·46
	$\frac{12}{12}$	90·0	64·3	33·7	106·1	·26	·32^5	·40	·43
	$\frac{13}{12}$	97·5	67·9	32·3	112·0	·25	·32	·37	·41
	$\frac{14}{12}$	105·0	71·3	30·7	118·0	·25	·32	·35	·41
	$\frac{15}{12}$	112·5	74·5	28·6	123·9	·26	·33	·32^5	·42
	$\frac{16}{12}$	120·0	77·5	27·0	129·8	0·27	0·34	0·31	0·46
$\phi \quad +55$	$\frac{7}{12}$	52·5	52·6	35·0	69·9	0·34	0·35	0·50	0·53
$\delta \quad +20$	$\frac{8}{12}$	60·0	56·7	36·5	77·0	·29	·32	·46	·48
	$\frac{9}{12}$	67·5	61·0	37·3	83·6	·26	·30	·42	·44
	$\frac{10}{12}$	75·0	65·3	37·6	89·6	·23	·28^5	·38	·40
	$\frac{11}{12}$	82·5	69·6	37·4	95·9	·22	·27	·35	·38
	$\frac{12}{12}$	90·0	73·7	36·7	101·8	0·20^5	0·26	0·32^5	0·37

	$\frac{l}{n}$	t	z	ω	A	$\frac{1}{4D}$	$\frac{C}{4D}$	$\frac{2E}{4D}$	ρ_ϕ
$\phi = + 50°$	$\frac{5}{12}$	37°·5	50°·6	30°·4	50°·9	0·41	0·37	0·49	0″·58
$\delta = + 10$	$\frac{6}{12}$	45·0	54·5	33·9	58·8	·33	·33	·45	·50
	$\frac{7}{12}$	52·5	58·8	36·6	66·0	·27^5	·30	·41^5	·45
	$\frac{8}{12}$	60·0	63·3	38·5	72·7	·24	·27	·38	·41
	$\frac{9}{12}$	67·5	68·0	39·8	79·0	·21	·25	·35	·37
	$\frac{10}{12}$	75·0	72·7	40·5	85·0	·19	·24	·32	·36
	$\frac{11}{12}$	82·5	77·5	40·7	90·8	·17	·22	·30	·36
	$\frac{12}{12}$	90·0	82·4	40·4	96·5	0·16	0·21	0·28	0·36
$\phi = + 45$	$\frac{5}{12}$	37·5	55·9	31·3	47·4	0·34	0·33	0·42	0·49
$\delta = \quad 0$	$\frac{6}{12}$	45·0	60·0	35·3	54·6	·27	·29	·39	·44
	$\frac{7}{12}$	52·5	64·5	38·4	61·5	·23	·26	·36	·40
	$\frac{8}{12}$	60·0	69·3	40·9	67·8	·19	·24	·33	·36
	$\frac{9}{12}$	67·5	74·3	42·7	73·7	·17	·22	·31	·35
	$\frac{10}{12}$	75·0	79·3	44·0	79·3	0·15	0·20	0·29	0·35
$\phi = + 40$	$\frac{3}{12}$	22·5	54·1	21·2	27·7	0·51	0·45	0·40	0·57
$\delta = - 10$	$\frac{4}{12}$	30·0	57·2	27·1	35·9	·37	·35	·38	·49
	$\frac{5}{12}$	37·5	60·8	32·3	43·3	·28	·30	·36	·44
	$\frac{6}{12}$	45·0	65·0	36·7	50·2	·23	·26	·34	·39
	$\frac{7}{12}$	52·5	69·6	40·4	56·4	·19	·23	·32	·36
	$\frac{8}{12}$	60·0	74·6	43·5	62·2	·16	·21	·30	·35
	$\frac{9}{12}$	67·5	79·8	46·0	67·6	0·13	0·19	0·28	0·33
$\phi = + 35$	$\frac{3}{12}$	22·5	59·0	21·5	24·8	0·42^5	0·42	0·35^5	0·52
$\delta = - 20$	$\frac{4}{12}$	30·0	61·9	27·6	32·2	·31	·33	·34	·45
	$\frac{5}{12}$	37·5	65·5	33·2	39·0	·23^5	·27	·33	·40
	$\frac{6}{12}$	45·0	69·6	38·2	45·1	·19	·24	·31	·36
	$\frac{7}{12}$	52·5	74·1	42·5	50·8	·15	·21	·30	·35
	$\frac{8}{12}$	60·0	79·2	46·2	55·9	0·13	0·19	0·28	0·34
$\phi = + 30$	$\frac{3}{12}$	22·5	63·7	21·7	21·7	0·35	0·39	0·32	0·48^5
$\delta = - 30$	$\frac{4}{12}$	30·0	66·5	28·2	28·2	·25	·30	·31	·42
	$\frac{5}{12}$	37·5	69·8	34·2	34·2	·19	·25	·30	·38
	$\frac{6}{12}$	45·0	73·7	39·6	39·6	·15	·22	·29	·36
	$\frac{7}{12}$	52·5	78·1	44·6	44·6	0·12	0·19	0·28	0·35
$\phi = + 25$	$\frac{2}{12}$	15·0	66·5	14·8	12·5	0·43^5	0·54	0·30	0·58
$\delta = - 40$	$\frac{3}{12}$	22·5	68·3	21·9	18·4	·28	·37	·30	·47
	$\frac{4}{12}$	30·0	70·7	28·7	23·9	·20	·28^5	·29	·41
	$\frac{5}{12}$	37·5	73·8	35·2	29·0	·15^5	·24	·28	·39
	$\frac{6}{12}$	45·0	77·3	41·0	33·7	0·12	0·20^5	0·27^5	0·37
$\phi = + 20$	$\frac{2}{12}$	15·0	71·3	14·9	10·7	0·34	0·52	0·28	0·58
$\delta = - 50$	$\frac{3}{12}$	22·5	72·8	22·1	14·9	·22	·35	·28	·49
	$\frac{4}{12}$	30·0	74·9	29·1	19·5	·16	·27	·27^5	·45
	$\frac{5}{12}$	37·5	77·5	35·9	23·6	·12	·22^5	·27	·42
	$\frac{6}{12}$	45·0	80·5	42·3	27·4	0·09	0·19^5	0·26	0·40

	$\frac{l}{n}$	t	z	ω	A	$\frac{1}{4D}$	$\frac{C}{4D}$	$\frac{2E}{4D}$	ρ_ϕ
$\phi = + 15°$	$\frac{2}{12}$	15°·0	76°·0	14°·9	7°·7	0·25	0·50	0·27	0″·63
$\delta = - 60$	$\frac{3}{12}$	22·5	77·2	22·3	11·3	·16	·34	·27	·55
	$\frac{4}{12}$	30·0	78·9	29·5	14·8	·12	·26	·26	·50
	$\frac{5}{12}$	37·5	80·9	36·6	18·0	·09	·22	·26	·48
	$\frac{6}{12}$	45·0	83·3	43·4	20·8	0·07	0·19	0·26	0·48
$\phi = + 10$	$\frac{2}{12}$	15·0	80·6	15·0	5·1	0·16^{5}	0·49	0·26	0·76
$\delta = - 70$	$\frac{3}{12}$	22·5	81·5	22·4	7·6	·11	·33	·26	·70
	$\frac{4}{12}$	30·0	82·6	29·8	9·9	·08	·25^{5}	·26	·67
	$\frac{5}{12}$	37·5	84·0	37·1	12·1	·06	·21	·25^{5}	·65
	$\frac{6}{12}$	45·0	85·7	44·3	14·0	0·04	0·18	0·25	0·62

Es sind jetzt die zufälligen Fehler der hier vorausgesetzten Beobachtungen zu ermitteln. Für diesen Zweck habe ich während der letzten Sommerferien mit dem Universalinstrumente von Repsold auf der Leidener Sternwarte (beschrieben in den Annalen Bd. I. p. XLIX.) eine Zahl von Höhen- und Azimuthunterschieden beobachtet. Das, was erhalten wurde, ist nun leider sehr unter dem, was ich zu erreichen hoffte, zurückgeblieben, und man hat in den gefundenen wahrscheinlichen Fehlern eher eine Grenze zu erblicken, die man selbst bei ungünstigen Umständen nicht überschreiten wird, als eine definitive Bestimmung der zu erreichenden Genauigkeit. Dies wird aber für die vorliegende Untersuchung genügen.

Das weniger zufriedenstellende der erhaltenen Resultaten ist hauptsächlich den ausserordentlich schlechten Witterungsverhältnissen des vorigen Sommers zuzuschreiben. Dadurch sah ich mich bald gezwungen entweder die Messung der Höhen- oder die der Azimuthunterschiede gänzlich aufzugeben. Ich beschloss sofort die ersteren aufzuopfern, von deren Genauigkeit sich leichter auf andere Weise eine genäherte Vorstellung gewinnen lässt.

Bei dem Mangel an direct beobachteten Höhenunterschieden habe ich eine Reihe von Bestimmungen absoluter Zenithdistanzen benutzt, die ich Aug. 27 bis Sept. 16, 1875 an demselben Instrumente machte. Aus jeder Z.D. ist mit den Auwers'schen Sternörtern die Polhöhe abgeleitet. Die Biegung wurde nach der Formel 1″·05 sin z berechnet, die sich aus der Vergleichung der nördlichen und südlichen Sterne ergab. Zunächst wurden nur die 86 Beobachtungen in Betracht gezogen, welche in Höhen über 30° angestellt sind. Diesen allen wurde das nämliche Gewicht beigelegt, und so gefunden für eine Beobachtung der Polhöhe, welche auf eine einzige Einstellung in den beiden Lagen des Instruments beruht, und bei einer mittleren Z.D. von 40°: w. F. = ± 0″·46. Hierin ist der w. F. eines Catalogorts mit inbegriffen, sowie auch noch die Theilungs und Schraubenfehler. Für eine Bestimmung der w. F. einer Z.D. unabhänging von den Oertern des Fundamentalcatalogs ist das Material sehr wenig geeignet. Aus den 29 Beobachtungen von λ ursæ minoris (welcher öfters sehr schwach war) wird 0″·41 gefunden.

Aus den Abweichungen der einzelnen Resultate von dem Tagesmittel jedes Sterns wird (unabhängig von Refraction und Theilungsfehlern) 0″·43 gefunden, aber mit Ausschluss einer durch Wolken gemachten Beobachtung von λ ursæ min. 0″·37. Im Mittel darf man 0″·43 annehmen. Aus den Polhöhenbeobachtungen auf Rügard, auf dem Seeberge und auf dem Inselberge (Astr. geod. Arbeiten in den Jahren 1872, 1869, 1867) mit einem Universalinstrumente von nur wenig grösseren Dimensionen finde ich: w. F. unabhänging von den Declinationen und den Theilungsfehlern, aus Beobachtungen an demselben Abend, = ± 0″·40 (183 Beob., mittl. Z.D. 37°).

Dieser w. F. wird mit der Z.D. zugleich wachsen. Ausgenommen bei sehr kleinen Entfernungen vom Zenith darf wohl angenommen werden, dass diese Zunahme dem nämlichen Gesetze folgt als bei den Beobachtungen mit einem Meridiankreise, deren Genauigkeit so ziemlich der hier gefundenen gleichkommt. An Sternwarten, die mehr als 30° vom Equator entfernt sind, werden diese Beobachtungen nie in Zenithdistanzen anzustellen sein, die erheblich über 65° hinausgehen; wird daher für den w. F. die Form $r^2 = a^2 + b^2 \operatorname{tg}^2 z$ angenommen, so braucht b nicht mit der höchsten Genauigkeit bekannt zu sein. Ich nehme dafür den Werth, welcher in Leiden (Annalen II, p. 145) bei Beobachtungen, deren w. F. ± 0″·49 für 40° Z.D., gefunden wurde. Mit diesem Werthe von b (der mit dem in Greenwich und anderswo gefundenen übereinstimmt) und dem Werthe 0″·43 für 40° Z.D. ergiebt sich—

$$r = \sqrt{(0{\cdot}396)^2 + (0{\cdot}20 \operatorname{tg} z)^2}$$

Dass diese Formel, wenigstens bei nicht gar zu grossen Z.D., noch ziemlich richtige Werthe giebt, wird durch die Beobachtungen über 60° vom Zenith wahrscheinlich gemacht. Diese beziehen sich mit nur drei Ausnahmen alle auf α ursæ maj. U.C. Die 13 Beobachtungen dieses Sternes ergeben für 65° 25′ Z.D. w. F. = ± 0″·60, indem die Formel 0″·59 giebt.

Der w. F. des Unterschiedes von zwei willkürlichen Zenithdistanzen zu willkürlichen Zeiten angestellt ist $r\sqrt{2}$. Gewiss aber wird der w. F. kleiner ausfallen bei der Messung des Unterschiedes von zwei fast gleichen Zenithabständen unmittelbar nach einander oder gewissermaassen sogar zu gleicher Zeit angestellt (wenn die Beobachtungen des nördlichen (N) und südlichen (S) Sterns einander in der Ordnung $N.S.S.N.$ folgen). Da sich aber dieser Umstand nicht in Rechnung bringen lässt, nehme ich auch für diese Messung der w. F. $r\sqrt{2}$ an, wiewohl man dadurch gewiss unter der Genauigkeit bleibt, die man bei der wirklichen Ausführung finden wird. Der wahrscheinliche Fehler einer einzigen Beobachtung eines Höhenunterschiedes, der auf zwei Einstellungen jedes der beiden Sterne beruht, ist demnach—

$$\rho_\beta = \sqrt{2}\,\sqrt{(0{\cdot}396)^2 + (0{\cdot}20 \operatorname{tg} z)^2}$$

Wiewohl bei den Beobachtungen des vorigen Jahres die Messungen von Höhenunterschieden aufgeopfert wurden, um dadurch Zeit für die Bestimmung von Azimuthunterschieden zu gewinnen, sind dennoch auch diese nicht nach Hoffnung gelungen. An jedem Abende, an welchem auch nur ein kleiner Theil des Himmels unbewolkt war, ist die Beobachtung versucht; dennoch sind nur an 12 von 27 Abenden eine oder mehr Beobachtungen gelungen. Nachdem schon während einiger Zeit beobachtet war, stellte es sich heraus, dass der Boden des Beobachtungslocals mit dem Pfeiler, auf dem das Instrument aufgestellt ist, in Berührung war; ein Uebelstand, wodurch in den Resultaten leicht constante Fehler entstehen konnten. Leider versäumte ich es eine Notiz über die Zeit zu machen, wann dies bemerkt wurde, und es bleibt daher in dieser Hinsicht eine kleine Ungewissheit. Wenn ich nicht irre, fällt dieser Datum zwischen Juli 25 und August 10; die zwei Tage 23 und 24 Juli müssen also jedenfalls ausgeschlossen werden. Die Tage Juli 26, 28, 29, August 3 habe ich alle beibehalten, da es hier bloss darauf ankommt, die w. F. der Beobachtungen zu bestimmen; es bleiben also noch 10 Abende übrig. Darunter ist aber einer mit nur einer einzigen Beobachtung, während noch an fünf der übrigen beständig mit Wolken zu kämpfen war, wodurch viele Beobachtungen verloren gingen und manche weniger gut gelangen; an einem Abend (12 Aug.) war die Luft ausserordentlich schlecht. Es bleiben dann nur drei Abende übrig, über welche in Hinsicht auf die Witterung nicht zu klagen war. Schon hierdurch hat nicht nur die Quantität sondern auch die Qualität der Beobachtungen leiden müssen. Aber auch noch andere Momente haben dazu wahrscheinlich beigetragen. Nachdem schon eine nicht unbeträchtliche Zahl von Beobachtungen angestellt war, zeigte eine nähere Betrachtung der Resultate grosse Anomalien in den Nivellirungen. Bei einer besonders dazu angestellten Untersuchung stellte es sich bald heraus, dass diese von der Kraft abhängig sind, mit welcher die Klemme der verticalen Achse festgeschraubt wird. Zwischen den Nivellirungen bei ziemlich losem und denjenigen bei sehr festem Anziehen der Klemmschraube zeigten sich Unterschiede, die bei gewissen Ablesungen bis über $5''\cdot5$, bei anderen bis über $2''\cdot4$ in entgegengesetztem Sinne gehen, während bei noch anderen Ablesungen solche Unterschiede nicht bestehen. So wie zu erwarten war, zeigt zugleich die Excentricität des Horizontalkreises eine grosse Veränderlichkeit. Diese Anomalie war übrigens schon früher bemerkt, vergl. die Berichten der Sternwarte 1872–73, p. 16; 1873-74 p. 5.

Zwar ist diese Anomalie, wenn nur zwischen dem Registriren der Fadenantritte, der Ablesung der Microscope und der Nivellirung der Achse keine Aenderungen im Instrumente vorgehen, ohne nachtheiligen Einfluss, aber es scheint mir, dass bei so grossem Spielraum der verticalen Achse kleine Aenderungen in der Lage des Instruments *während* dieser Operationen nicht unmöglich sind. Diesem Uebelstande war aber nicht abzuhelfen, und ich sah mich darauf angewiesen, die Stellen der Achse, wo die Abweichungen am grössten waren, ausser Gebrauch zu lassen, dabei ziemlich lose zu klemmen, und die Microscope zweimal abzulesen, das eine Mal vor, das andere Mal nach der Nivellirung und der Beobachtung der Fadenantritte. Es hat sich herausgestellt, dass dadurch weitere Anomalien fast ganz vermieden sind.

Die Einrichtung der Beobachtungen war nun die folgende. Für $\frac{l}{n}$ wurde $\frac{2}{3}$ genommen. Jede vollständige Bestimmung der Polhöhe wird daher auf die Beobachtung von sechs südlichen und drei nördlichen Sternen beruhen. Nachdem eine beträchtliche Zahl solcher Systeme von je neun Sternen aufgesucht waren, wurden die Azimuth-Einstellungen von jedem zusammen gehörigen Paar nach den Formeln (32) bis (35) berechnet, wobei für die Zeit λ der constante Werth 10^{m} genommen wurde. Bei jeder der zwei zusammen gehörigen Durchgänge wurde dann, wo möglich, 15 Fadenantritte registrirt, nivellirt und die beiden Microscope abgelesen. Zwischen den zwei Durchgängen musste das Dach gedreht, die zweite Einstellung gemacht, u.s.w. Die Antritte am verticalen Faden wurden immer sehr nahe in der Mitte zwischen den 20″ von einander entfernten horizontalen Faden beobachtet.

An jedem Abend (mit Ausnahme des 26sten Juli, an welchem Tage nur eine Beobachtung gelang) wurde Polaris einmal in den beiden Kreislagen eingestellt zur genäherten Bestimmung der Meridianablesung. Schliesslich bemerke ich, dass drei Mal während der ganzen Beobachtungsreihe, von Juli 26 an, der Horizontalkreis jedesmal um ungefähr 72° 12′ verdreht wurde.

Mit der gefundenen Meridianablesung wurden die Ablesungen in Azimuthe verwandelt, und diese als Näherungen der absoluten Azimuthe betrachtet. Für ϕ^{0} wurde 52° 9′ 20″·0 genommen und für die δ^{0} Werthe abgeleitet aus den für 1882·0 angenommenen mittleren Oertern. Mit diesen Näherungen konnten nun nach den Formeln die Werthe von $\lambda - \lambda^{0}$ berechnet werden, die noch für den Gang der Uhr und die Veränderungen in der Lage des Instruments während der zehn Minuten verbessert wurden.

Von zehn Sternpaaren sind (nach Ausschluss der Tage Juli 23 und 24) im Ganzen 72 Beobachtungen erhalten, so dass im Mittel jedes Paar sieben Mal beobachtet ist. Davon habe ich eine einzige, aus unbekannten Gründen stark abweichende, ausgeschlossen. Von den übrigen ist sechs Beobachtungen, die entweder auf sehr wenigen Fadenantritten beruhen, oder die aus anderen Gründen, im Journale angegeben, als unsicher zu betrachten waren, halbes Gewicht zuertheilt. Aus den verschiedenen Werthen für $\lambda - \lambda^{0}$, welche jedes Sternpaar ergab, wurde nun der w. F. einer solchen Bestimmung abgeleitet. Das einfache Mittel aus zehn Einzelresultaten ist $0^{s}{\cdot}0477 + 0{\cdot}0047$. Das Resultat bei einer Gewichtsvertheilung im Verhältniss zu der Zahl der Beobachtungen ist $+ 0^{s}{\cdot}0481$ (oder 0″·626, wenn der Fehler ganz der Beobachtung der h aufgebürdet wird). Dabei war das mittlere Azimuth 77°, Z.D. 59°·5, Stundenwinkel 61°, Decl. + 16°, Parall. Winkel 39°.

Um aus diesem Resultate für einen besonderen Fall, auch für andere Fälle Werthe abzuleiten, die wenigstens eine genäherte Vorstellung der zu erwartenden Genauigkeit gewähren, ist es gut den ganzen Fehler der Beobachtung eines Azimuthunterschiedes derart über λ und h zu vertheilen, dass unter w. F. von λ (hier mit ρ_λ bezeichnet) der reine Durchgangsfehler verstanden wird, m. a. W. der eigentliche Pointirungsfehler in dem Unterschiede der Durchgangszeiten der beiden Sterne, während der w. F. von h (ρ_h) die Ablesungsfehler, die Theilungsfehler, die übrig gebliebenen Schraubenfehler, die Fehler der Nivellirungen bei der Beobachtung beider Sterne umfassen soll. Vor Allem sind daher die ρ_h und ρ_λ *getrennt* für die vorliegenden Beobachtungen zu berechnen. Zu diesem Zweck leitete ich die folgenden w. F. ab—

w. F. eines einzelnen Fadenantrittes, ...	$0^{\mathrm{s}}{\cdot}063$
Daraus, weil im Mittel etwa elf Antritte erhalten wurden:	
w. F. eines einzelnen Durchgangs, ...	$0{\cdot}019$
w. F. einer Ablesung an zwei Microscopen (Ablesefehler + Theilungsfehler + übrig gebliebenen Schraubenfehler),*	$0''{\cdot}26$
w. F. einer doppelten Nivellirung, ...	$0{\cdot}31$

Der mittlere Niveaucoefficient ist cot 59°·5 = 0·59, man hat daher—

$$\rho_h = \sqrt{2[(0{\cdot}26)^2 + (0{\cdot}31 \cdot 0{\cdot}59)^2]} = 0''{\cdot}45$$
$$\rho_\lambda = 0^{\mathrm{s}}{\cdot}019\,\sqrt{2} = 0^{\mathrm{s}}{\cdot}027$$

Der totale w. F. von zwei zusammen gehörigen Gliedern der Formel (22), welche sich auf die nämliche Beobachtung beziehen, also von—

$$-(\lambda_{k,k+1} - \lambda^{\circ}_{k,k+1}) + \tfrac{1}{2}(C_{k,w} + C_{k+1,o})(h_{k,k+1} - h^{\circ}_{k,k+1}),$$

m. a. W. der totale w. F. einer $\lambda - \lambda^{\circ}$, wenn der ganze Fehler der Messung eines Azimuthunterschiedes dieser Grösse aufgebürdet wird, ist daher—

$$\sqrt{\rho^2_\lambda + [\tfrac{1}{2}(C_{kw} + C_{k+1,o})\,\rho_h]^2}.$$

Der mittlere Werth des Coefficienten von ρ_h war aber 1·153, man erhält also schliesslich der totale w. F. der Bestimmung eines Azimuthunterschiedes = ± $0^{\mathrm{s}}{\cdot}0439$, ein Werth, der nicht unerheblich von dem direct gefundenen $0^{\mathrm{s}}{\cdot}0481$ abweicht. Schliesst man aber die ersten Beobachtungstage aus, an welchen, wie oben erwähnt wurde, noch andere als die genannten Fehlerquellen befürchtet werden müssen, so erhält man statt $0^{\mathrm{s}}{\cdot}0481$, nach August 3: $0^{\mathrm{s}}{\cdot}0460$ (50 Beob.). Dies stimmt schon sehr befriedigend mit dem auf anderen Weg gefundenen Werthe. Ich gebe aber dem direct gefundenen Werth $0^{\mathrm{s}}{\cdot}0481$ den Vorzug, weshalb der Unterschied beider Bestimmungen gleichmässig über ρ_h und ρ_λ vertheilt wird. Man erhält $\rho_\lambda = 0^{\mathrm{s}}{\cdot}0295$, $\rho_h = 0''{\cdot}496$, wofür in runden Zahlen $0^{\mathrm{s}}{\cdot}03$ und $0''{\cdot}50$ genommen wurde.

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* Unabhängig von den Schraubenfehlern wurde 0″·223 gefunden; für die übrig gebliebenen Schraubenfehler, die nicht mit der grössten Schärfe bestimmt sind, wurde dieser Werth ziemlich willkürlich auf 0″·26 gebracht.

Es ist nun zunächst der Werth dieser Grössen für andere Zenithdistanzen, Declinationen u.s.w. abzuleiten. Der Werth von ρ_h wird voraussichtlich von den veränderten Umständen nur in geringem Maasse beeinflusst werden, und jedenfalls wird man den hier gefundenen Werth wohl nie beträchtlich zu vergrössern haben, denn man wird meistens die Azimutheinstellungen nicht in Zenithdistanzen anstellen, die erheblich kleiner als 60° sind, und der Niveaucoefficient wird deshalb nur in seltenen Fällen ein wenig grösser als der hier auftretende sein. Die Ablesungs, Theilungs und Schraubenfehler bleiben unberührt. Ich nehme daher für alle Fälle $\rho_h = 0''{\cdot}50$.

Der Durchgangsfehler dagegen wird sich erstens mit der Declination δ und dem parallactischen Winkel ω änderen. Setzt man—

$$\cos\nu = \cos\delta\cos\omega \tag{38}$$

so ist ν die Declination eines Sternes, der im Meridian in der nämlichen Zeit das Fadennetz durchläuft als es der Stern von der Decl. δ bei ihrem schiefen Durchgange thut. Es scheint deswegen gestattet für den w. F. die Form $a\sqrt{1+b^2\sec^2\nu}$ anzunehmen. Die Beobachtungen sind zu einer Bestimmung von b gar nicht geeignet; ich nehme dafür den Werth 0·24, welcher für die Meridiandurchgänge in Pulkova gefunden wurde. Der Coefficient a ergiebt sich dann aus dem oben für $\delta = 16°$, $\omega = 39°$, also $\nu = 42°$ gefundenen Werth von ρ_λ. Man findet—

$$\rho_\lambda = 0''{\cdot}43\sqrt{1+(0{\cdot}24\sec\nu)^2} \tag{39}$$

So lange ν unter 60° ist, variirt dieser Werth von ρ_λ so wenig, dass ich zwischen $\nu = 0$ und $\nu = 60°$ den constanten Werth $\rho_\lambda = 0''{\cdot}45$ annahm.

Ausser mit ν wird aber auch ρ_λ sich zugleich mit der Z.D. ändern. Die hier benutzten Beobachtungen sind in Zenithdistanzen angestellt, die von 54° bis 67° gehen (so weite Grenzen sind bei der unten gegebenen Modification der Methode erlaubt). Die Durchgangsfehler sind dabei in den grösseren Zenithdistanzen nicht grösser als in den kleineren. Ich betrachte daher den abgeleiteten Werth für gültig bis zu 70° Z.D.; weiter nahm ich an (zwar sehr willkürlich), dass dieser Fehler für 80° Z.D. zweimal, für 85° dreimal grösser wird. Im ganzen wurden also nachfolgende Werthe angenommen—

	ρ_λ		
Z.D.	$\nu = 0°$ bis $60°$	$\nu = 70°$	$\nu = 80°$
40°	0''·45	0''·52	0''·73
70	0·45	0·52	0·73
80	0·90	1·04	1·46
85	1·35	1·55	2·19

ρ_h
0''·50

ϕ	z	ρ_β
60°	30°	0''·58
50	40	·61
40	50	·65
30	60	·73
20	70	·96
10	80	1·70

Diese Werthe hat man nur noch in die oben tabellarisch zusammengestellten Ausdrücke für $d\phi$ einzusetzen, welche allgemein diese Form haben—

$$n\,d\phi = a\Sigma(\lambda-\lambda^\circ) + b\Sigma(h-h^\circ) + c\Sigma(\beta-\beta^\circ) \tag{40}$$

Eine vollständige Beobachtungsreihe besteht aus $2n$ Beobachtungen eines Azimuthunterschiedes, und $2n$ Beobachtungen eines Höhenunterschiedes. Der w. F. eines $d\phi$ bestimmt durch eine solche Reihe von $4n$ Beobachtungen wird somit sein—

$$\frac{1}{n}\sqrt{2na^2\rho_\lambda^2 + 2nb^2\rho_h^2 + 2nc^2\rho_\beta^2} \tag{41}$$

Will man diesen w. F. für das Resultat von $4n$ Beobachtungen mit dem w. F. vergleichbar machen, der für $n = 1$ gefunden wird. d. h. mit dem w. F. des Resultats von nur vier Beobachtungen, so hat man ihn noch mit $\sqrt{n}$ zu multipliciren. Die Genauigkeit des Resultats einer *gleichen* Zahl von Beobachtungen wird man daher nach dem obigen Werth multiplicirt mit $\sqrt{n}$ beurtheilen können: welche Grösse ich daher mit ρ_ϕ bezeichne. Man kann sich dieses so vorstellen, als wäre ρ_ϕ der w. F. einer $d\phi$, welche auf zwei gemessenen Azimuthunterschieden und zwei gemessenen Höhenunterschieden beruht. Es ist aber—

$$\rho_\phi = \sqrt{2}\sqrt{\left(\frac{1}{4D}\right)^2\rho_\lambda^2 + \left(\frac{C}{4D}\right)^2\rho_h^2 + \left(\frac{2E}{4D}\right)^2\rho_\beta^2} \tag{42}$$

welcher nach der oben gegebenen Tabelle berechnet werden kann. Die gefundenen Werthe sind in der letzten Spalte der erwähnten Tabelle eingetragen.

IV. *Auswahl der zweckmässigsten Werthe für $\frac{l}{n}$.*

Die Z.D., in welcher die Höhenmessungen anzustellen sind, wird im Mittel der Colatitude ziemlich gleich sein, welcher Werth von $\frac{l}{n}$ auch immer angenommen wird. Nur die Z.D. der Einstellungen bei den Azimuthbeobachtungen hängt von dieser Wahl ab. Man wird es gewiss vermeiden in gar zu grosser Nähe des Horizonts zu beobachten, wie weit man aber noch schicklich gehen kann, werde ich nicht entscheiden. Daher muss die Wahl des passendsten Werthes von $\frac{l}{n}$ dem Beobachter überlassen bleiben, weshalb ich der obigen Tafel eine grosse Ausführlichkeit gab. Nur die folgenden allgemeinen Bemerkungen glaube ich mir erlauben zu dürfen.

(*a*) Der Werth von n ist möglichst klein zu nehmen, denn, wie sofort einleuchtet, wird eine ungleichmässige Vertheilung der Zahl von Beobachtungen über die verschiedenen zu messenden Grössen um so nachtheiliger sein, je grösser n ist.

(*b*) Der wahrscheinliche Fehler von ϕ soll möglichst klein sein; wie aus der Tafel ersichtlich wird dieser im allgemeinen kleiner bei wachsender Z.D. (wenigstens bis sehr grossen Z.D.)

(*c*). Gar zu kleine und gar zu grosse Zenithabstände bei den Azimuthbeobachtungen sind zu vermeiden. Denn einerseits werden nicht blos die zufälligen Fehler (deren Zunahme mit der Z.D. schon bei der Ableitung der w. F. berücksichtigt wurde), sondern auch die systematisch auf die Messung der λ und h einwirkenden Fehlerquellen, wofern solche noch bestehen, bei wachsender Z.D. wahrscheinlich zunehmen, andererseits aber nehmen die Coefficienten, nicht nur der λ und h sondern auch die der β, bei wachsender Z.D. sehr rasch ab. Wenn daher auch unbekannte Fehlerquellen auf die Messungen der λ und h, in grösseren Entfernungen vom Scheitel, vielleicht keinen geringen Einfluss ausüben, so wird doch jedenfalls eine grössere Unabhängigkeit von den systematischen Fehlern der Höhenmessungen gewonnen.

Es giebt aber noch ein anderer sehr wichtiger Grund, um den grösseren Zenithdistanzen den Vorzug zu geben. Dieser trifft zu, wenn man zur Bestimmung absoluter Declinationen die Δ_k durch Beobachtungen grösster Azimuthe bestimmt; dennoch glaube ich diesen Grund hier nicht unerwähnt lassen zu können. Gesetzt also, die Δ_k (folglich die $d\Delta_k$) seien bekannt. Die Gleichungen (15) ergeben dann sofort die Declinationen δ_k der Sterne des Gürtels, für dessen Mitte $\delta = 2\phi - 90°$. Ich behaupte nun, dass durch Vergrösserung der Z.D., in der die Azimuthbeobachtungen angestellt werden: *erstens* der Einfluss etwaiger constanter Fehler in den Beobachtungen der Höhenunterschiede β (z. B. ein Unterschied der nördlichen und südlichen Refractionen) auf jede der so bestimmten δ_k beliebig verkleinert werden kann; und *zweitens* dass dies auch der Fall ist mit dem Einflusse *aller* Fehler in den Beobachtungen der Höhenunterschiede auf dass Mittel aller so bestimmten δ_k. Denn führt man den Werth von $d\phi$ (23) in (15) ein, so wird—

$$(43)\quad d\delta_k = -\,d\Delta_k - \frac{1}{2nD}\,\Sigma(\lambda - \lambda^\circ) + \frac{C}{2nD}\,\Sigma(h - h^\circ) - \frac{2E}{2nD}\,\Sigma(\beta - \beta^\circ) + \beta_k - \beta^\circ_k$$

$$(44)\qquad \Sigma d\delta_k = -\,\Sigma d\Delta_k - \frac{1}{D}\,\Sigma(\lambda - \lambda^\circ) + \frac{C}{D}\,\Sigma(h - h^\circ) + \left(1 - \frac{2E}{D}\right)\Sigma(\beta - \beta^\circ)$$

Haftet nun in (43) allen β der nämliche Fehler $\Delta\beta$ an, und wird der daraus für δ_k entspringende Fehler mit $\Delta\delta_k$ bezeichnet, so ist offenbar—

$$(45)\qquad \Delta\delta_k = \left(1 - \frac{2E}{D}\right)\Delta\beta$$

Nach den Formeln (19) und (20) aber ist—

$$(46)\qquad 1 - \frac{2E}{D} = \frac{\cos z \cos \delta}{2\cos\phi - \cos z \cos \delta}$$

welcher Werth durch Vergrösserung von z beliebig klein gemacht werden kann. Die Richtigkeit der aufgestellten Behauptung geht daraus unmittelbar hervor. Der Werth von (46) für beliebige Polhöhe und Z.D. ist aus der oben gegebenen Tafel, in welcher die Werthe von $\frac{2E}{4D}$ eingetragen sind, leicht zu ersehen. Der Einfluss der constanten Fehler in den Messungen der β auf die δ lässt sich also sehr klein machen. Dafür müssen aber die Beobachtungen der Azimuthe in hoher

Z.D. angestellt werden. Der Nachtheil, der daraus für diese Messungen selbst in Bezug auf etwaige systematische Fehler entsteht, wird (wenigstens theilweise) wie bei der Polhöhenbestimmung selbst, durch die kleineren Coefficienten der λ und h compensirt.

Wiewohl es, ohne eine nähere Kenntniss der Natur der vielleicht noch übrigbleibenden systematischen Fehler, unmöglich ist die Z.D. der Azimutheinstellungen zu bestimmen, welche mit Berücksichtigung der übrigen Umstände als die geeignetste zu betrachten ist, so geht doch aus dem angeführten hervor, dass man sich wahrscheinlich irren würde, wollte man bei diesen Beobachtungen nicht über die mittleren Zenithdistanzen hinausgehen. Bei weitem am besten aber ist es gewiss sich nicht auf einen einzigen Werth von $\frac{l}{n}$ zu beschränken, sondern wenigstens zwei verschiedene Werthe zu benutzen. Man erhält dadurch verschiedene Bestimmungen der Polhöhe und der Declinationen, die in verschiedenem Maasse von allen möglicherweise noch übrig bleibenden systematischen Fehlern abhängig sind.

Schliesslich stelle ich einige geeigneten Werthe von $\frac{l}{n}$ übersichtlich zusammen, dabei hat n nur die Werthe 1 bis 4, der w. F geht nicht über 0″·45 hinaus, die Z. D. bei den Azimuthbeobachtungen nicht über 80°—

	$\frac{l}{n} = \frac{4}{3}$		$\frac{l}{n} = \frac{5}{4}$		$\frac{l}{n} = 1$		$\frac{l}{n} = \frac{3}{4}$		$\frac{l}{n} = \frac{2}{3}$		$\frac{l}{n} = \frac{1}{2}$	
ϕ	z	ρ_φ	z	ρ_φ	z	ρ_φ	z	ρ_φ	z	ρ_φ	z	ρ_φ
60°	77°·5	0″·46	74°·5	0″·42	64°	0″·43						
56					72	·38						
52					79	·36	65°	0″·40	61°	0″·43		
48							71	·36	66	·39		
44							75·5	·35	70	·36	61°	0″·43
36									78	·34	69	·37
28											75	·37

Ich will hier eine Bemerkung einschalten, die für gewisse Polhöhen sehr wichtig sein kann. Für Polhöhen über 45° ist es möglich einen solchen Werth für $\frac{l}{n}$ auszuwählen, dass das Azimuth der extrameridionalen Beobachtungen nicht gar zu sehr von 90° verschieden ist und statt einer Bestimmung des Azimuthunterschiedes wird man sodann die Durchgänge durch den ersten Vertical beobachten können mittelst eines gewöhnlichen Passageinstruments. Will man dabei aber grössere Werthe von n vermeiden, so muss man in vielen Fällen eine längere Zeit λ zwischen der östlichen und westlichen Beobachtung verlaufen lassen. Für $\frac{l}{n} = 1$ und $\phi = 52°$ hat man z. B. $\lambda = 1^h\ 30^m$; für $\frac{l}{n} = \frac{3}{4}$ und $\phi = 60°$ ist $\lambda = 0^h\ 24^m$. Diese grösseren Werthe von λ verursachen natürlich eine viel grössere Abhängigkeit von den Aenderungen der Meridianablesung und von dem Uhrgange. Von den Unregelmässigkeiten der Zapfen und von der seitlichen Biegung aber bleibt man frei, wenn nur die Beobachtungen gleichmässig über die beiden Lagen des Instruments vertheilt werden. Wie schon erwähnt und sogleich gezeigt werden soll, kann durch eine leichte Umförmung der Methode, bei der Beobachtung der Höhenunterschiede auch die Talcottsche Methode in Anwendung kommen. In den grösseren Polhöhen wird es somit möglich sich gänzlich von allen Kreisablesungen zu befreien. Der Kürze halber beschränke ich mich auf diese Andeutung.

V. *Zweite Form der Methode.*

Die bis jetzt besprochene Methode leidet an gewissen Uebelständen. Man hat die südlichen Sterne innerhalb eines sehr engen Gürtels zu wählen, dessen Breite nicht über ein Paar Grade hinausgeht. Wenn aber zur Polhöhenbestimmung derjenige der absoluten Declinationen der benutzten Polarsterne angeschlossen wird, so sind es gerade diese südlichen Sterne, deren absolute Declinationen sich überdies ohne weiteres ergeben. Man wird sich daher in diesem Falle die Breite des Gürtels so gross als möglich wünschen.

Die Höhenunterschiede, die man zu messen hat, sind nicht unbeträchtlich. Wie früher bemerkt, können dieselben etwa bis 6° resp. 10° steigen, je nachdem man keine schwächere Sterne als $8^{m}\cdot0$ oder $7^{m}\cdot0$ beobachten will. Bei kleineren Polhöhen wird man deshalb die Unsicherheiten der Refraction nicht mehr immer als gänzlich beseitigt betrachten können. Ganz besonders ist es aber zu bedauern, dass die Grösse dieser Unterschiede die Anwendung der Talcott'schen Methode unmöglich macht.

Diesen Ubelständen lässt sich aber vollständig abhelfen. Ich werde dieses für die zwei speciellen Fälle $\frac{l}{n} = 1$ und $\frac{l}{n} = \frac{2}{3}$ zeigen; in allen übrigen Fällen kann man auf ähnliche Weise verfahren.

$\frac{l}{n} = 1$.—Eine vollständige Combination von Sternen besteht aus: einem Polarstern und zwei südlichen Sternen, deren R.A. nahe um 180° verschieden sind. Gewöhnlich wird man natürlich mehrere dieser Combinationen beobachten. Bei geeigneter Wahl derselben wird man sich von den dA und $d\Delta$ in hohem Maasse unabhängig machen können, ohne dass man die Wahl der südlichen Sterne auf einen schmalen Gürtel zu beschränken hat. So wie man aber von dieser Bedingung frei wird, hindert weiter nichts mehr, die südlichen Sterne so zu wählen, dass ihre Z.D. im Meridian derjenigen der Polsterne in oberer und unterer Culmination gleichkommt. Man hat, um dieses zu erreichen, nur die Combinationen paarweise so zu wählen (wie man es sich leicht durch eine Figur veranschaulicht)—

Σ_1	$\Delta_1 \ \alpha_1$	Σ_2	$\Delta_1 \ \alpha_1 + 180°$
S_1	$\delta_1 \ \alpha_1$	S_3	$\delta_1 \ \alpha_1 + 180$
S_2	$\delta_2 \ \alpha_1 + 180°$	S_4	$\delta_2 \ \alpha_1$

wo δ_1 und δ_2 einen Unterschied von 12° oder selbst 20° haben können, und weiter so gewählt sind, dass man nur *kleine* Höhenunterschiede zu messen hat, also δ_1 wenig verschieden von $2\phi - \Delta$, δ_2 wenig verschieden von $2\phi = (180 - \Delta)$.

Denn werden die Azimuthunterschiede in diesen beiden Serien, also—

$$A_{1,o} - A_{2,w} \quad \text{und} \quad A_{4,o} - A_{3,w}$$

gleichfalls—

$$A_{2,o} - A_{1,w} \quad \text{und} \quad A_{3,o} - A_{4,w}$$

an dem nämlichen Abend beobachtet, so überzeugt man sich sofort, dass die Coefficienten des letzten Gliedes der beiden Gleichungen von der Form (22), die man erhält, nämlich—

$$C_{2,w} - C_{1,o} \quad \text{und} \quad C_{3,w} - C_{4,o}$$
$$C_{1,w} - C_{2,o} \quad \text{und} \quad C_{4,w} - C_{3,o}$$

den gleichen Betrag, aber mit entgegengesetztem Vorzeichen, haben, so dass der Einfluss von Fehlern in den angenommenen Oertern des Meridians in dem Mittel der beiden Bestimmungen von $d\phi$ verschwindet.

Auch die Coefficienten von $d\Delta_1$ und $d\Delta_2$ werden nahe gleich sein, aber mit dem nämlichen Vorzeichen. Im Mittel der zwei Bestimmungen wird also ausser $d\phi$ nur die Unbekannte $d\Delta_1 + d\Delta_2$ vorkommen. Der Coefficient dieser Unbekannten wird immer noch klein sein, und eine Bestimmung von $\Delta_1 + \Delta_2$, wenngleich diese nicht auf die höchste Genauigkeit Anspruch machen kann, wird für die Bestimmung von $d\phi$ ausreichen. Eine solche Bestimmung von ausreichender Genauigkeit wird aber aus den gemachten Kreisablesungen gewonnen, weil die beiden Polarsterne 12^h in R.A. verschieden sind und also in entgegengesetzten Culminationen unmittelbar nach einander zur Einstellung gelangen. Wird aber die Talcott'sche Methode in Anwendung gebracht, so wird eine besondere Bestimmung von $\Delta_1 + \Delta_2$ erfordert, die zwar immer sehr leicht ist, aber doch eine gewisse Vermehrung der Arbeit verursacht.

Diese Elimination wird aber viel einfacher und zu gleicher Zeit wird sehr erheblich an Symmetrie der Beobachtungen gewonnen, wenn man nicht den Unterschied im Azimuth der Sternpaare S_1, S_2; S_3, S_4 misst, deren Declinationen bedeutend verschieden sind, sondern statt dessen diesen Unterschied bestimmt bei den Sternpaaren S_1, S_3; S_4, S_2, deren Declinationen einander ziemlich gleich sind. In diesem Fall wird eine vollständige doppelte Combination also die folgenden Beobachtungen umfassen—

Z.D. Σ_1, O.C. — Z.D. $S_1 = \beta_1$ Z.D. Σ_1, U.C. — Z.D. $S_2 = \beta_2$
Z.D. Σ_2, U.C. — Z.D. $S_4 = \beta_4$ Z.D. Σ_2, O.C. — Z.D. $S_3 = \beta_3$

$A_{3,w} - A_{1,o} = h_{3,1}$ Beob. Zeit $S_{3,w}$ — Beob. Zeit $S_{1,o} = \lambda_{3,1}$
$A_{2,w} - A_{4,o} = h_{2,4}$ „ $S_{2,w}$ — „ $S_{4,o} = \lambda_{2,4}$
$A_{1,w} - A_{3,o} = h_{1,3}$ „ $S_{1,w}$ — „ $S_{3,o} = \lambda_{1,3}$
$A_{4,w} - A_{2,o} = h_{4,2}$ „ $S_{4,w}$ — „ $S_{2,o} = \lambda_{4,2}$

Diese Beobachtungen ergeben die nachfolgenden zwei Gleichungen, die man vollständig wie (22) ableitet—

$$\begin{aligned} d\phi\,[D_{1.w} + D_{3.w} - D_{1.o} - D_{3.o}] = & -(\lambda_{3.1} - \lambda^{\circ}_{3.1}) - (\lambda_{1.3} - \lambda^{\circ}_{1.3}) \\ & + \tfrac{1}{2}(C_{3.w} + C_{1.o})\,(h_{3.1} - h^{\circ}_{3.1}) + \tfrac{1}{2}(C_{1.w} + C_{3.o})\,(h_{1.3} - h^{\circ}_{1.3}) \\ & - (E_{1.w} - E_{1.o})\,(\beta_1 - \beta^{\circ}_1) - (E_{3.w} - E_{3.o})\,(\beta_3 - \beta^{\circ}_3) \\ & + (E_{1.w} - E_{1.o})\,d\Delta_1 + (E_{3.w} - E_{3.o})\,d\Delta_2 \\ & + \tfrac{1}{2}(C_{1.w} - C_{3.o})\,(dA_{1.w} + dA_{3.o}) + \tfrac{1}{2}(C_{3.w} - C_{1.o})\,(dA_{3.w} + dA_{1.o}). \end{aligned}$$

$$\begin{aligned} d\phi\,[D_{2.w} + D_{4.w} - D_{2.o} - D_{4.o}] = & -(\lambda_{2.4} - \lambda^{\circ}_{2.4}) - (\lambda_{4.2} - \lambda^{\circ}_{4.2}) \\ & + \tfrac{1}{2}(C_{2.w} + C_{4.o})\,(h_{2.4} - h^{\circ}_{2.4}) + \tfrac{1}{2}(C_{4.w} + C_{2.o})\,(h_{4.2} - h^{\circ}_{4.2}) \\ & - (E_{2.w} - E_{2.o})\,(\beta_2 - \beta^{\circ}_2) - (E_{4.w} - E_{4.o})\,(\beta_4 - \beta^{\circ}_4) \\ & - (E_{2.w} - E_{2.o})\,d\Delta_1 - (E_{4.w} - E_{4.o})\,d\Delta_2 \\ & + \tfrac{1}{2}(C_{2.w} - C_{4.o})\,(dA_{2.w} + dA_{4.o}) + \tfrac{1}{2}(C_{4.w} - C_{2.o})\,(dA_{4.w} + dA_{2.o}) \end{aligned}$$

Diese zwei Gleichungen geben jetzt nicht mehr jede für sich eine Bestimmung von $d\phi$, denn die Coefficienten der dA sind zwar verschwindend, die Coefficienten der $d\Delta$ aber sind ganz von der nämlichen Ordnung wie die Coefficienten der übrigen Grössen. Durch Verbindung der beiden Gleichungen aber wird man Δ_1 und Δ_2 beide fortschaffen können, wenn—

$$\frac{E_{1.w} - E_{1.o}}{E_{3.w} - E_{3.o}} = \frac{E_{2.w} - E_{2.o}}{E_{4.w} - E_{4.o}}$$

ohne dass dadurch der Coefficient von $d\phi$ weniger günstig wird. Dieses wird nun u.a. Statt finden, wenn $\delta_1 = \delta_3$, $t_1 = t_3$, $\delta_2 = \delta_4$, $t_2 = t_4$.

Aber auch ohne die vollkommene Erfüllung dieser Bedingungen, und zwar innerhalb solcher Grenzen, die wenigstens eben so weit genommen werden dürfen als bei den früheren Beobachtungen, kann man sich in dieser Weise von den Fehlern in den angenommenen Δ_1 und Δ_2 (und zugleich von den absoluten Azimuthen) frei machen. Wünscht man die Talcott'sche Methode anzuwenden, so wird man sich die Beobachtungsobjecte folgendermassen wählen: zwei Polarsterne deren Declinationen nicht mehr als etwa 2°, deren R.A. aber 12^h verschieden sind, und vier südliche Sterne, die mit diesen Polarsternen nahe zu gleicher Zeit (innerhalb weniger Minuten) und in gleicher Z.D. (innerhalb weniger Bogenminuten) in den Meridian gelangen.

$\frac{l}{n} = \frac{2}{3}$.—Die Zahl der Sterne einer vollständigen Combination beträgt nicht weniger als 9, d.h. drei Polsterne und sechs südliche Sterne. Es ist daher ein glücklicher Umstand, dass in diesem Falle schon eine einzige Combination den verlangten Zweck erreichen lässt (im allgemeinen tritt dieser Umstand immer ein, wenn der Quotient, welchen man erhält, wenn l durch den grössten gemeinschaftlichen Theiler von l und n dividirt wird, eine gerade Zahl ist).

Man wähle die Polarsterne derart, dass die oberen Culminationen gleichmässig über die 24^h vertheilt liegen, und ihre Declinationen innerhalb ein Paar Grade einander gleich sind. Es seien also die R.A. der Sterne Σ_1, Σ_2, Σ_3 resp. α, $8^h + \alpha$, $16^h + \alpha$, die der südlichen Sterne α, $4^h + \alpha$, $8^h + \alpha$, $12^h + \alpha$, $16^h + \alpha$, $20^h + \alpha$. Die Sterne S_1, S_3, S_5 werden sich dann im Meridian befinden zur Zeit der O.C. der Sterne Σ_1, Σ_2, Σ_3. Wählt man daher diese südlichen Sterne derart, dass sie resp. in gleicher Z.D. (innerhalb weniger Minuten) culminiren als die Sterne Σ_1, Σ_2, Σ_3, so wird man erstens die Talcott'sche Methode in Anwendung bringen können, zweitens aber werden auch δ_1, δ_2, δ_3 innerhalb ein Paar Grade einander gleich, und bei der Bestimmung der Azimuthunterschiede der Sternpaare S_1S_3, S_3S_5, S_5S_1 werden die beiden Sterne eines Paares so nahe symmetrisch zum Meridian stehen, dass der Werth von Coefficienten wie $\frac{1}{2}(C_{1,w} - C_{3,o})$ (vergl. (22)) fast verschwindend ist.

Ebenso kann man die Declinationen der Sterne S_2, S_4, S_6, die im Meridian zur Zeit der U.C. der Sterne Σ_1, Σ_2, Σ_3 gelangen, derart wählen, dass man die Höhenunterschiede nach der Talcott'schen Methode bestimmen kann; bei dieser Wahl werden die δ_2, δ_4, δ_6 einander bis auf ein Paar Grade gleich, und der Werth von Coefficienten wie $\frac{1}{2}(C_{2,w} - C_{4,o})$ verschwindet.

Weiter aber ist in diesen beiden Gruppen die Summe der R.A.'s Unterschiede der Null gleich; die Beobachtungen von jeder der beiden Gruppen giebt daher eine Gleichung von der Form (22), in welcher die dA nur verschwindende Coefficienten haben, übrigens aber die vier unbekannten Grössen $d\phi$, $d\Delta_1$, $d\Delta_2$, $d\Delta_3$ enthalten sind. Die Coefficienten dieser drei letzteren sind aber in beiden Gleichungen (wenn wenigstens auch die μ klein gehalten werden, vergl. Formel 22 und 29) nur sehr wenig verschieden. Demzufolge lässt sich aus diesen beiden die Grösse $d\Delta_1 + d\Delta_2 + d\Delta_3$ in der Weise eliminiren, dass die resultirende Gleichung nur noch die $d\Delta_1$, $d\Delta_2$, $d\Delta_3$ mit äusserst kleinen Coefficienten enthalten wird. Man kann sich daher in beliebigem Maasse von den Fehlern in den angenommen Δ und absoluten Azimuthen frei machen.

In diesem Aufsatze wurde fast ausschliesslich die Polhöhenbestimmung in's Auge gefasst, und nur im Vorbeigehen auch der Declinationsbestimmung gedacht. Fast immer aber ist diese letztere die Hauptsache, und in diesem Falle ist bei der gefolgten Beobachtungsweise nicht sofort ersichtlich, wie viel *besondere* Arbeit auf die Polhöhenbestimmung zu verwenden ist, ja es ist vielleicht dadurch der Eindruck hervorgebracht, dass diese besondere Arbeit sehr beträchtlich ist. Dem ist aber nicht so, und ein Beispiel wird dies anschaulich machen. Gesetzt, man habe sich die Bestimmung der absoluten Declinationen von 42 Sternen nach dem oben (p. 148) angedeuteten Verfahren zur Aufgabe gestellt, vovon 10 in dem Gürtel (*a*), 20 in dem Gürtel (*c*), und 6 in jedem der Gürtel (*b*, *d*) und (*e*) gewählt sein mögen. Die Polhöhe des Beobachtungsortes sei 55°. Will man die w. F. der Declinationen

der 10 Circumpolarsterne auf 0″·1 herunterbringen und auch die Höhenunterschiede mit einem w. F. von 0″·1 kennen lernen, so werden für diese Bestimmungen, die jedenfalls anzustellen sind (auch wenn die Polhöhe schon bekannt wäre), mit dem Leidener Instrumente etwa 1600 Beobachtungen erforderlich sein (bei Anwendung der Talcott'schen Methode allerdings eine geringere Zahl). Aber die Kenntniss der Polhöhe unabhängig von der Refraction u.s.w. wird gewöhnlich schon durch 100 besonders dazu angestellten Beobachtungen mit einem w. F. unter 0″·04 gefunden. Diese besonders auszuführende Arbeit ist daher keine sehr bedeutende.

SUR LA QUEUE MULTIPLE DE LA COMÈTE DE 1744.

Par THÉODORE SCHWEDOFF.

Dans ma Note "Sur les configurations des Comètes" j'oppose à l'hypothèse des forces répulsives ce fait que la direction des conoïdes de la grande comète de 1744, calculés par M. Bredichin dans cette hypothèse, dévie de 30° à 60° de leur direction observée par Chéseaux le 7 mars 1744 à Lausanne. L'Astronome de Moscou m'objecte qu'une théorie ne peut point être ébranlée par quelques observations isolées et douteuses et que les observations faites sur la même comète à Berlin et publiées récemment par M. Dreyer l'affermissent dans l'opinion que le dessin de Chéseaux, pour le jour indiqué ci-dessus, est erroné. Il renvoie le lecteur à l'article de M. Dreyer, particulièrement au mots: "The Berlin drawings agree fairly well with the theory etc." (p. 107). Cette fois j'ai en vue de montrer que le dessin de Mademoiselle Kirch n'a pas le sens qu'on veut lui attribuer.

Au premier examen de ce dessin, on ne tarde pas à s'apercevoir que, même si l'on fait abstraction des deux ou trois bandes les plus australes, celles qui restent sont fortement convergentes vers l'horizon, de sorte que les deux bandes extrêmes forment entre elles un angle de plus de 35°. Au contraire, les conoïdes théoriques, représentés sur le dessin de M. Bredichin (Annales de l'Obs. de Moscou VII., 1) sont presque parallèles dans la partie qui pouvait être visible le 7 mars 1744 en Europe. Par l'effet de la perspective, les rayons extrêmes de M. Bredichin, visibles au dessus de l'horizon, n'auraient formé qu'un angle de 8° au plus.

Une étude détaillée du dessin de Berlin nous montre que les bandes qui s'y trouvent s'accordent avec les conoïdes de M. Bredichin aussi peu que celles de Lausanne. Je vais éclaircir ce point au risque d'être prolixe.

Nous avons, jusqu'à présent, trois observations de la comète de 1744 qui se rapportent toutes au 7 mars: celle de Chéseaux, celle de De L'Isle et le dessin de Mlle. Kirch. Les deux premières ont été reproduites par M. Winnecke sur une carte céleste dans le Bulletin de l'Académie de St. Petersbourg (1864), de sorte qu'on peut déterminer la direction des bandes de la comète en mesurant sur la carte les angles de position. Mais ce procédé ne serait point applicable au dessin de Mlle. Kirch, parce que les étoiles y sont réparties par approximation et leur position ne pourrait convenir rigoureusement à un réseau de sphère céleste. A défaut des courbes méridiennes sur ce dessin, je fixe la position des bandes qui s'y trouvent en rapportant leur directions à celle d'une droite menée par les étoiles β et ϵ du Pégase. Ces étoiles offrent cet avantage sur les autres, que la droite menée par elles coupe toutes les bandes du dessin. Je mesure les angles A formés par les bandes avec cette droite, ainsi que les distances D de l'étoile β aux points d'intersection des bandes avec la dite droite, la distance $\beta\epsilon$ étant prise pour l'unité. Ensuite, en prenant les distances D pour abscisses et les angles A pour ordonnées, je dresse une courbe qui représente, approximativement sans doute, la variation dans les directions des bandes, en partant de l'est vers l'ouest.

J'applique ce procédé au dessin de M. Winnecke (l'étoile β du Pégase ne se trouve pas sur ce dessin, mais on n'a aucune peine à en fixer la position), ainsi qu'aux bandes observées à Berlin et aux bandes théoriques calculées dans l'hypothèse des forces répulsives. La table ci-dessous représente la confrontation de la théorie avec l'observation—

	A			
D	De l'Isle.	Chéseaux.	Kirch.	Théorie.
0·38			40°	23°
0·53			38	23
0·54	46°			23·5
0·65			43	23·5
0·67	45			23·5
0·85	47	47°		24
0·88			43	24·5
1·05	44			24·5
1·06	42	54		25
1·16			50	25
1·31		63		25
1·37			62	25
1·52		81		25
1·58			79	25
1·66			78	25·5
1·73		95		25·5
1·77			79	25·5
1·85			77	25·5
1·91		110		26

Comme on le voit bien, les trois observations offrent un accord auquel on ne pouvait pas s'attendre, vu le caractère confus du phénomène. La divergence des courbes ne s'observe qu'à partir de la distance $D = 1·6$, laquelle correspond aux deux bandes de Chéseaux et aux trois bandes de Mlle. Kirch les plus courtes et par conséquent les plus difficiles à fixer. On voit aussi, si la théorie est confirmée par le dessin de Berlin. Et si c'est "surtout dans la comète de 1744" que doit se manifester la proprieté de ces conoïdes, que doit on penser des propriétés d'autres comètes.

Peut-être m'objectera-t-on cette fois encore qu'une théorie ne peut point être ébranlé par une observation isolée et douteuse. Mais alors je ne conçois pas comment on pourrait se tirer de cette alternative: ou bien une observation est douteuse, et alors elle ne peut pas servir d'appui à n'importe quelle théorie ; ou bien on veut s'en emparer pour s'affermir complètement dans une opinion, et alors on n'a plus le droit d'en mettre en doute la justesse.

Je crois devoir aux lecteurs de ce Journal une explication touchant les "quelques mots" de M. Bredichin (p. 125). Cette fois il s'agit du désaccord entre la théorie de ce savant et la figure observée de la comète de Donati. J'ai dit que "la direction des conoïdes dans la partie supérieure de cette comète dévie de 20° à 50° de leur direction théorique."—"Or, réplique M. Bredichin, comment M. Schwedoff obtient-il ces déviations? Il compare ma figure citée, qui est calculée pour $q = 0{\cdot}25$, avec la queue de la comète Donati, ou $q = 0{\cdot}6$, et la valeur de q a une influence très grande sur la courbure de la queue. Le lecteur ne se laissera pas induire en erreur et il saura apprécier ce genre de polémique."

M. Bredichin a tort. Les déviations dont il s'agit n'ont pas été obtenues par moi dans la supposition $q = 0{\cdot}25$ comme le fait croir mon adversaire, mais pour $q = 0{\cdot}578$, distance périhélie de la comète Donati. Et si je cite la figure de la comète-type à $q = 0{\cdot}25$, ce n'est plus pour obtenir les déviations, mais bien pour relever ce "fait que d'après la théorie des forces répulsives la direction des conoïdes est *toujours* longitudinale par rapport à la direction générale de la queue, tandis qu'en réalité elle à été dans ces deux comètes (1744 et 1858 V.) transversale." Et immédiatement j'ajoute: "qu'on compare la queue-type de M. Bredichin. . . . "

FURTHER NOTE ON THE COMET OF 1744.

Since my paper on the multiple tail of the Great Comet of 1744 was published, I have had an opportunity of examining Chéseaux's original work. The reproduction of the plate representing the tails of the comet in Professor Winnecke's paper (Mélanges math. et astron. T. III.) is very poor, as it does not show the peculiar double appearance of five of the tails. On Chéseaux's plate the tails are very bright along the edges and much fainter in the middle, which is in good accordance with his text. In the latter he mentions expressly that the sixth (most southerly) tail was single. "Cette sixième, jointe aux dix bandes plus éclatantes des autres, formoit l'apparence d'once rayons;" this is perfectly intelligible when one looks at Chéseaux's own engraving but loses its meaning when compared with the copy. A comparison between Margaretha Kirch's drawing (this journal, Pl. IV.) and

Chéseaux's plate shows at once that the eleven rays on the former are identical with the five double tails and one single tail on the latter. Chéseaux also makes the third tail much broader than the rest, which agrees with the interval between Kirch's fifth and sixth rays being wider than the other intervals. The Berlin drawing thus seems to have been carefully made and not to be a mere rough sketch, as I had at first supposed.

In Chéseaux's text the words: "La quatrième queue se terminoit à une ligne qui passoit par α Aquilæ et α Delphini," evidently refer to the third and not to the fourth tail.

J. L. E. D.

RECENT LITERATURE.

L'Aberration des Étoiles fixes. Par Magnus Nyrén (Mém. de l'Acad. Imp. de St. Pétersbourg, 1883, 47 pp. 4to). The value of the constant of Aberration found by W. Struve has been in universal use for many years, not only on account of the high authority of its author, but also owing to the confidence the general plan of the work and the small probable error ($\pm 0''\cdot011$) of the result could not fail to inspire. Other recent investigations have however made it probable that Struve's constant must be affected by a much larger error than indicated by the amount of the probable error, as they agree in giving the constant a value which is several hundredths of a second larger. On the other hand it is not easy to point out the sources of error in Struve's observations; there are only two suppositions which may be doubtful, viz. the regularity of the daily clock-rate and the invariability of the azimuth of the Prime-Vertical Transit Instrument. W. Struve examined these circumstances himself some years after the publication of his researches, and felt obliged to add a small correction to the original value of the constant and adopt a somewhat larger probable error. These questions have later on been considered by O. Struve, who came to the result that a new determination of the constant of Aberration was desirable. Although an irregularity of the clock-rate can hardly have much influence on the result, on account of the shortness of the interval between the East- and West-Transit of a star, the adopted method of determining the azimuth by observing stars of large Zenith Distance made it necessary to trust to the invariability of the azimuth during many hours, and it is evident that periodic motions of the instrument might produce considerable systematic errors in the results.

In the Connaissance des Temps for 1878 M. Villarceau has examined the influence of the proper motion of the Solar System on the value of the constant of Aberration.

The motion of the Sun in space as determined by observations is only its motion with regard to the Galactic System, and as it seems probable that its annual amount does not much exceed the diameter of the Earth's annual orbit, the maximum influence on the constant of Aberration would be 0″·0006. As to the absolute motion of the Galactic System, we are completely in the dark, but even supposing it to be ten or twenty times as great as the Sun's relative motion, its influence on the constant of Aberration, when determined from stars in various parts of the heavens, would be quite insensible.

The instrument used by Dr. Nyrén is the same as that employed by Struve, the Transit Instrument in the Prime Vertical. In the interval between the two series of observations a new micrometer has been constructed for the instrument, having two groups of seven fixed wires separated from the middle wire by intervals of 110″, and the single wires of each group being 37″ (or two revolutions of the screw) distant from each other. Instead of one movable wire there are now two, also about 37″ apart; they were only employed as fixed wires, *i.e.* placed in a certain position and left there during the whole transit of a star across one branch of the Prime Vertical. Stars culminating more than a degree from the Zenith were observed over the movable wires, placed outside the fixed ones, and the transits over the two wires nearest the middle one were omitted, as only in this way the transit could be observed, the level read, the instrument reversed, the level read again and the second transit over the same wires observed. In the case of stars within 15′ of the Zenith, the two outer wires were omitted and the movable wires were placed approximately in the middle of two intervals of fixed wires, care being taken to eliminate the value of a revolution of the screw by making the reading of the drum exactly the same for the four transits of the star over the same wire. The illumination has been re-arranged so as to do away with the possibility of systematic errors in the Zenith distances being caused by it. A new level has been provided and has always been left on the axis and only reversed with the instrument. In order thoroughly to control the constancy of the azimuth, two marks with collimating lenses (of respectively 400 and 500 feet focal length) have been erected East and West of the instrument and have been observed by means of the movable micrometer-wires. During the whole investigation the marks were read before and after each complete observation, and it became at once apparent that the azimuth was anything but constant, as the variations often amounted to one or two seconds in the course of two hours. By comparing the readings made on several consecutive days, an analogous variation was generally (though far from always) apparent, and from a table in which Dr. Nyrén has put together the monthly means of hourly variations in the mornings and evenings, it appears that the positive sign was remarkably prevalent (*i.e.* that the south end of the axis moved from East to West), and that the evening variations were nearly always greater than the morning ones. The signs and amounts being generally the same, the corrections to be introduced in the present investigation were but very small, but it is of course quite possible that the case would have been different with Struve's observations, and the negative parallaxes resulting from the latter might possibly in this way be explained satisfactorily, as they increased with the Zenith distances of the Stars. Dr. Nyrén is inclined to seek the explanation of the variations of the Azimuth in the rise of temperature in the pier of the instrument caused by the proximity of the observer's body. In computing the corrections to the observations, the variations were assumed to be proportional to the time, an assumption which was borne out by the facts, whenever the Azimuth marks were read repeatedly in the course of a few hours.

The regularity of the clock rate was tested by comparisons of the clock used for the observations with the Standard Clock of the Observatory before and after each observation. Thus, the results are free from the uncertainty which may attend Struve's, both as regards Azimuth and clock rate. The stars employed are also more numerous, twenty-four instead of seven. They are all situated between + 59° 37′ and + 58° 29′ Declination and are fairly uniformly distributed in R.A. The observations were commenced on the 17th December, 1879, and were finished on the 29th January, 1881, they are 566 in number and comprise two maxima and two minima of Aberration for each star. In the equations of condition the parallax of the star was introduced (though the observations were made at epochs when the coefficient of the parallax was nearly zero), also the correction to the adopted mean declination. The equations having been solved, the Author takes the mean of the corrections thus deduced for every six hours of R.A. as the observations are not uniformly distributed in R.A. (though the stars are). The mean results are—

$0^h - 6^h$	Corr. =	$+ 0''{\cdot}066$	$+ 0{\cdot}52\, \pi_0$	$\pm 0''{\cdot}026$
$6 - 12$		$- 0{\cdot}067$	$- 0{\cdot}02\, \pi_0$	$\pm 0{\cdot}040$
$12 - 18$		$+ 0{\cdot}163$	$+ 0{\cdot}16\, \pi_0$	$\pm 0{\cdot}025$
$18 - 24$		$+ 0{\cdot}127$	$+ 0{\cdot}13\, \pi_0$	$\pm 0{\cdot}019$

where π_0 is the mean parallax of the stars observed. From the high degree of accuracy of the single observations a better agreement between the four mean results might have been expected. It thus seems that the corrections found are not quite free from systematic error, and this is further proved by a table, in which the mean monthly deviations (obs. — calc.) for all the observations near a maximum or minimum of Aberration are put together, as there are clearly marked systematic changes in the deviations. It remains as yet undecided whether these are due to a temporary change of the observer's personal error or of the latitude of the observatory, or are caused by lateral refraction. But as the arithmetical means of the coefficients of Aberration near maximum and near minimum, taken separately for the four seasons of the year, are very near zero, periodic variations in the observed Declinations extending over some months will not appreciably affect the mean result for the constant of Aberration. The simple mean of the four results given above, Corr. = $+ 0''\cdot072 + 0\cdot20\,\pi_0 \pm 0''\cdot014$, must therefore be regarded as the final result, and π_0 may safely be assumed equal to zero, the stars being mostly under the fifth magnitude. This gives the Constant = 20″·517.*

Dr. Nyrén has also discussed a series of observations of three circumpolar stars (*α* and *δ* Ursæ min., 51 Cephei) made by M. Wagner during the years 1861–72 with the large Transit Instrument, both by Eye and Ear and with Chronograph. The three stars give the corrections + 0″·039, + 0″·024 and + 0″·033, mean value = + 0″·038 and Constant = 20″·483. Finally Dr. Nyrén considers it safest to combine all the values of the constant found in Pulkova—

by Vertical Circle (Peters, Gyldén, Nyrén), ...	20″·495
by Transit Instr. (Schweizer, Wagner), ...	20·491
by Pr. Vert. Transit (W. Struve, Nyrén), ...	20·490

The mean is 20″·492 ± 0″·006, to which corresponds a solar parallax of 8″·777 or 8″·791, according as Cornu's or Michelson's determination of the velocity of light is adopted.

The Parallax of α Lyræ and 61 *Cygni. By Asaph Hall* (Wash. Obs. 1879, App. 1, 64 pp. 4to). As remarked by the author, a new determination of the parallax of any star by a new observer with a new instrument is of value, as the period of parallax is a year, during which time the instrument and probably also the observer undergo changes which have the same period. Professor Hall observed differences of declination of *α* Lyræ and its companion of the 10th magnitude with the 26-inch Equatoreal from May 24, 1880, to July 2, 1881, on 77 nights and 61 (2) Cygni and the 9·5 mag. star 3′ south from October 24, 1880, to December 7, 1881, on 66 nights. In observing *α* Lyræ two kinds of illumination were used, the bright wires being denoted by *B* and the dark wires in bright field by *A*. For 61 Cygni only the bright field was used. The total effect of changes of temperature on the objective, tube, and micrometer screw was investigated by measuring the difference of declination of the stars No. 5 and No. 12 of Krüger's Catalogue of ths cluster *h* Persei. This difference is 18′ 38″, but by means of intermediate stars it can be measured with the same eyepiece that was used in the parallax observations. The following values of the annual parallax of *α* Lyræ were found—

with illum. *A*	$\pi =$ 0″·1556 ± 0″·00764
with illum. *B*	$\pi =$ 0·2080 ± 0·00827
Mean by weight:	$\pi =$ 0·1797 ± 0·005612

For 61 Cygni (2) the resulting parallax was 0″·4783 ± 0″·01381. Both results are in very good accordance with other recent determinations. At the end of the paper are given the results of a series of observations of *α* Lyræ with the Prime Vertical Transit Instrument at Washington, made from 1862 to 1867. Every care

was taken in making the observations (according to Struve's plan), but the resulting declinations of the star from the years 1863 to 1866 give a negative parallax. Possibly the heat from a stove in the adjoining library may have destroyed the accuracy of the results.

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* W. Struve's original value was 20″·445; he afterwards adopted 20″·463. See "Recueil de Mém. des Astronomes de Pulkova," T. I., p. xi.

A Determination of the Semi-Diameter of the Moon from two occultations of the Pleiades observed on July 6, 1877, *and September* 6, 1879. *By H. M. Paul.* (Wash. Obs. 1879. App. 2, 38 pp. 4to). These occultations were observed with the 9·6-inch Refractor and Chronograph at Washington, on both occasions the immersions took place on the bright limb and the emersions on the dark limb. The positions of the 19 stars occulted were obtained by adopting Newcomb's position of η Tauri (Catalogue of Clock and Zodiacal Stars) and applying C. Wolf's differential measures to it. A comparison between Bessel's heliometer measures and Wolf's wire micrometer measures was made use of to deduce separate proper motions for the stars, although the Author seems to have been aware of the probability (one might almost say certainty) that both the Königsberg and the Paris results are not free from systematic errors, the former at least so far as the less frequently observed stars are concerned, the latter chiefly near the north and south limits of the group. In 1877 the atmospheric conditions were not favourable and the Moon was in Zenith distances from 71° to 53°, besides which the stars were badly distributed round the limb. The only result of any interest which can be deduced from them is an apparent dependence of the semi-diameter on the magnitude of the stars, the brighter stars giving a smaller value. The author remarks, that this might be expected of the immersions on the bright limb, but so far as the dark limb is concerned, the result is less satisfactorily explained and is probably accidental. Seven occultations are however too few to derive a general law from. The observations in 1879 were made under very favourable conditions and the observations were considered excellent. The equations were solved both using Wolf's and Newcomb's positions for ten of the stars, and the former giving smaller residuals, the results deduced from them were alone adopted. Hansen's tables (including Newcomb's corrections) had been used for computing the places of the Moon, and the resulting corrections are—

Corr. to tabular semi-diameter: $-\ 1''\cdot63 \pm 0''\cdot12$
Corr. to tabular parallax: $+\ 1\cdot91 \pm 0\cdot77$
Corr. to tabular R.A. (Sept. 6·87): $-\ 8\cdot19 \pm 0\cdot19$
Corr. to tabular Decl. (Sept. 6·87): $-\ 0\cdot46 \pm 0\cdot28$

The correction to the mean semi-diameter becomes $-\ 1''\cdot69$ and the resulting semi-diameter is $15'\ 31''\cdot78 \pm 0''\cdot12$.

Annual Report of the Board of Regents of the Smithsonian Institution for the year 1881. (Washington 1883. 8vo.) This volume is considerably more bulky than the preceding ones, and probably for this reason the collection of reports from European and American Observatories, contained in the volumes for 1879 and 1880, has been omitted. This is much to be regretted particularly as regards the American Observatories, and it is to be hoped, that future volumes will contain condensed reports from these, omitting private "observatories" of small dimensions. The chapter of the "Record of scientific Progress" dealing with Astronomy is (as formerly) contributed by Professor Holden and fills 40 pages. We extract from it the following interesting particulars of *Professor Peters' Star Maps*, which to our knowledge have not been published elsewhere—

"The charts already *finished* comprise the space as given below—

R.A.		Decl.		R.A.		Decl.	
From $0^h\ 20^m$	to $0^h\ 40^m$	From + 9°	to + 10°	From $12^h\ 0^m$	to $12^h\ 20^m$	From − 5°	to + 10°
1 0	1 20	+ 10	+ 15	13 20	13 40	− 5	− 7
1 0	1 20	+ 5	− 1	13 40	14 0	− 10	− 15
1 20	1 40	+ 12	+ 15	14 0	14 10	− 10	− 15
1 20	1 40	0	+ 5	15 20	16 0	− 20	− 22
2 20	2 40	+ 17	+ 18	19 0	19 20	− 22	− 23
3 0	3 20	+ 15	+ 20	19 0	19 20	− 25	− 27
9 40	10 0	+ 9	+ 10	20 40	21 0	− 25	− 28
10 0	10 20	+ 10	+ 15	21 0	21 20	− 20	− 25
10 20	10 40	+ 12	+ 15	21 20	22 0	− 10	− 20
10 40	11 0	+ 10	+ 15	22 0	22 20	− 15	− 5
11 0	11 20	0	− 5	22 20	22 40	− 5	− 10
11 20	11 40	0	− 5	23 0	23 40	0	+ 3
11 40	12 0	− 2	− 5	23 40	24 0	− 4	− 6
11 40	12 0	+ 4	+ 10				

"Quite as many more charts are either 'begun' (with the catalogue stars laid down) or have the zones completed, needing only comparison with the sky. Some idea of the amount of this work may be had from the fact that already about 64,000 zone stars have been observed and mapped. Quite as many more of the fainter stars are needed to complete the maps, and perhaps half of these are now mapped. The charts of Chacornac and those of the Paris Observatory are extremely useful, but by no means so complete as those of Dr. Peters; and in general they do not cover the same place in the heavens. No. 31 of the Paris charts, however, occupies the same ground as one of Dr. Peters' charts. On May 17, 1878, I compared the Paris chart No. 31 with the sky, and at the same time I compared Dr. Peters' chart of the same region; and on the following day the two maps were compared together. The Paris chart No. 31 contains 1554 stars and extends from $10^h\ 0^m$ to $10^h\ 21^m$ R.A. and from + 8° 45′ to + 14° 0′ Decl.; the Hamilton College chart, extends from $10^h\ 0^m$ to $10^h\ 20^m$ R.A. and from + 10° to + 15° Decl. The number of stars laid down on the space common to both is, according to the Paris chart, 1010 stars, according to Dr. Peters' chart 1511 stars. I found more than thirty cases where the stars of the Paris charts were either much too large or too small, or where they occupied places where no stars now are. In all these cases the Hamilton College chart was correct. Errors of position I naturally could not verify in the time at my disposal, but considering the allineations of the stars as seen in the sky and by comparing these with the data of the Paris chart, I found the latter frequently incorrect, not always by small amounts. The configurations of the group of smaller stars are quite wrong in very many cases, even where all the stars are given; and very many stars are missing of a magnitude superior to the smallest included on the chart." [Instances of such errors are then given, after which Prof. Holden continues;] "In every one of these cases the Hamilton College Chart was correct, and after a careful examination of nearly every star on the latter chart I found only one case in which the chart seemed to me in the least erroneous. One faint star seemed about one magnitude too bright. This chart was not only an accurate map of the stars, it was also a picture of them, and a configuration of stars in the sky would at once catch the eye on the map and *vice versa*. It need hardly be said that this is not true of any other series of charts existing, not even of the maps of Argelander's Durchmusterung."

Astronomers who are using star charts will find the following *Index to Chacornac's and the later Paris charts*, compiled by Professor Holden, of use :—

No.	R.A.		Decl.		No.	R.A.		Decl.	
1	0^h 0^m to	0^h 20^m	+ 1° 0′ to	– 4° 15′	32	10^h 20 to	10^h 40^m	+ 6° 45′ to	+ 12° 0′
1 *a*	0 0	0 20	+ 1 0	+ 6 15	34	11 0	11 20	+ 2 45	+ 8 0
2	0 20	0 40	– 1 45	+ 3 30	35	11 20	11 40	+ 0 30	+ 5 45
2 bis	0 20	0 40	+ 3 30	+ 8 45	36	11 40	12 0	– 1 30	+ 3 45
3	0 40	1 0	+ 6 15	+ 5 30	39	13 0	13 20	– 10 30	– 5 15
3 *a*	0 40	1 0	+ 6 30	+ 10 45	41	13 20	13 40	– 12 0	– 6 45
4	1 0	1 20	+ 4 45	+ 10 0	43 *a*	14 0	14 20	– 10 30	– 5 15
5	1 20	1 40	+ 7 0	+ 12 15	46	15 0	15 20	– 20 30	– 15 15
6	1 40	2 0	+ 9 0	+ 14 15	49	16 0	16 20	– 23 45	– 18 30
7	2 0	2 20	+ 10 30	+ 15 45	50	16 20	16 40	– 24 30	– 19 15
9	2 40	3 0	+ 14 0	+ 19 15	51	16 40	17 0	– 25 0	– 19 45
13	4 0	4 20	+ 18 45	+ 24 0	52	17 0	17 20	– 25 30	– 20 15
15	4 40	5 0	+ 20 0	+ 25 15	60	19 40	20 0	– 23 45	– 18 30
22	7 0	7 20	+ 20 0	+ 25 15	61	20 0	20 20	– 22 45	– 17 30
26	8 20	8 40	+ 16 15	+ 21 30	62	20 20	20 40	– 21 30	– 16 15
27	8 40	9 0	+ 15 0	+ 20 15	63	20 40	21 0	– 20 15	– 15 0
28	9 0	9 20	+ 13 45	+ 19 0	64	21 0	21 20	– 22 0	– 16 45
29	9 20	9 40	+ 12 15	+ 17 30	64 *a*	21 0	21 20	– 16 45	– 11 30
29 *a*	9 20	9 40	+ 7 0	+ 12 15	70	23 0	23 20	– 8 0	– 2 45
30	9 40	10 0	+ 10 30	+ 15 45	71	23 20	23 40	– 5 45	– 0 30
31	10 0	10 20	+ 8 45	+ 14 0	72	23 40	24 0	– 3 45	+ 1 30

On the Influence of Magnetism on the Rate of a Chronometer. By O. Boeddicker (Trans. R. Dublin Soc. Vol. III. 56 pp. 4to). The question, whether and how far magnetism is able to influence the rate of a chronometer, is nearly as old as the invention of chronometers, and since in 1798 S. Varley published the first observations and experiments on the subject, it has occupied the attention and time of a considerable number of observers. Very little progress has however, so far, been made towards a final solution of the question, and the Author, considering that this chiefly arises from the investigators having had a very insufficient acquaintance with the observations previously made, has therefore in the present essay given a series of abstracts from and discussions of all the papers on magnetic influence on chronometers which have come to his knowledge. The well-known fact that chronometers frequently change their rates when transported from one place to another, is first alluded to, and a number of instances described, but of course no completeness is attempted. The magnetic state of the different steel parts in the work of a chronometer is next considered, and various methods are described which have been used to neutralize the magnetism, but very little appears to be known as to its distribution in the different steel parts, and the observations made only show the desirability of chronometer makers determining the situation of the magnetic poles of the various parts by dipping them into iron filings. The numerous observations of the rates of chronometers when under the influence of either artificial or terrestrial magnetism are next described, but although they prove that the proximity of a magnet or even a mass of soft iron effects the rate, no general results can as yet be derived from the experiments. Among the researches quoted, those of Sir G. B. Airy appear to be of special interest and importance (Nautical Magazine, 1840). The chronometer experimented on (Brockbank No. 425) was more subject to magnetic influence than any other up to that time tried at Greenwich. It had been sent back to the Royal Observatory in September, 1839, because its rate underwent changes when it was placed near an iron door. It was then during 29 days compared daily and after each comparison turned 90° in azimuth; and as each cycle of observations only occupied four days, it was considered probable that the gradual changes of rate peculiar to most chronometers would not sensibly affect the results. The mean rates found were—

Figure XII.	North	— $4^{s}{\cdot}64$
	East	— 8·70
	South	— 9·61
	West	— 5·71

from which the following formula for the daily rate G with XII. in any azimuth A was found: $G = -7^{s}{\cdot}16 + 2^{s}{\cdot}48 \cos A - 1^{s}{\cdot}50 \sin A$. Sir George Airy further states that the action of terrestrial magnetism upon a magnetic chronometer may be annihilated by placing the letter on the top of a compass box, whose needle is perfectly free, provided that the elevation of the magnetic parts above the compass is that, in which a small compass-needle, when placed above the large one, would lose its directive power. When accordingly the above-mentioned chronometer was placed above a large compass in such a way that its balance (in which the magnetism was assumed to be) was as nearly as possible at the same height as the small compass needle, the effect of terrestrial magnetism was counteracted.

In conclusion, the Author of the essay remarks, that further experiments are highly desirable, but that single chronometers are not likely to lead to general results. The compensation for the effects of temperature should be well determined, and the various kinds of experiments, in different azimuths, under the influence of soft iron and of magnets, ought all to be made, one after the other, on one and the same chronometer. Thus a relation between the effects of terrestrial and artificial magnetism and of soft iron (induced magnetism) might probably be found. It seems likely that a chronometer which is influenced by terrestrial magnetism will also (and only then) be affected by soft iron, and that on the other hand, a magnet will disturb any chronometer, no matter whether it is affected by the Earth and soft iron, or not.

AN ACCOUNT OF SOME RECENT ASTRONOMICAL EXPERIMENTS AT HIGH ELEVATIONS IN THE ANDES.

By RALPH COPELAND, Ph. D.

On the conclusion of the operations with which he was charged in connexion with the observations of the Transit of Venus in Jamaica in December, 1882, the writer arranged to visit South America with a view to testing the astronomical capabilities of the climate and elevated situation of Quito. This extension of the journey was rendered possible by a liberal clause in the instructions of the Transit observing parties which permitted any member of the same to return by any route provided the managing committee were not called upon to defray more than the cost of the direct return journey. The necessary outlay for the digression to South America was most liberally met by Lord Crawford. Letters of introduction were obligingly furnished by Sir Anthony Musgrave, then Governor of Jamaica, and by Captain Parkes of the Royal Mail Service, which proved of great use in procuring various facilities from the different Steamboat Companies : to Mr. J. W. Firth, Agent-General of the Pacific Steam Navigation Company, I am also indebted for much valuable aid and advice.

In view of the notorious difficulties of transport between the shores of the Pacific and Quito the outfit was restricted to the smallest available limit. It consisted of a 6-inch telescope of 74½ inches focus, the lens of which was figured by Simms, a few small accessories and some meteorological instruments which will be described further on. The fine Cooke equatorial mounting used at Jamaica was reluctantly left behind as far too heavy for mule transport. In its place a light 4-inch mounting, also by Cooke, with 6-inch clips was sent from Dunecht along with an automatic solar spectroscope by Browning. Delay through fog prevented the apparatus just named from arriving in time at Jamaica, but as the rainy season for the equatorial region of the Andes was close at hand, it was considered best to push on immediately, leaving instructions for the expected appliances to be forwarded. The Isthmus of Panama was crossed and Guyaquil, the great port of Quito, reached on New Year's Day, 1883. Unfortunately the whole country was in a state of revolution, thus rendering it to the last degree unlikely that the equatorial mounting could be sent on after me, as all transit would probably cease in a few days. My own journey to Quito indeed would have been practicable enough as a party of travellers thoroughly acquainted with the country most cordially offered to take me along with them. But not wishing to cut myself adrift from postal communication with the rest of the world the idea of visiting Quito was perforce abandoned. Two other routes to the Andes, but far to the south, remained to be tried. These were from Lima to Anchi at a height of 12220 feet on the Central Transandean Railway from Lima or even to an elevation of 14360 feet by way of the railway from Mollendo on the Pacific shore in south latitude 17° to Puno on the Lake of Titicaca.

At Lima it turned out that the upper part of the Transandean Railway was closed in consequence of the war between Peru and Chili. The whole of the littoral with the exception of the ports of Tumbez and Mollendo being held by the latter power, while the Peruvians retained the interior. There thus remained the choice between any of the numerous mule tracks into the higher districts and the Mollendo Railway. Fortunately in Lima I fell in with Mr. John L. Thorndike, the lessee of the last-named line of communication, who offered me every facility and assistance in his power. General Lynch, the Chilian commander-in-chief readily granted a permit to pass their blockade at Mollendo which port was reached by the end of January. Proceeding immediately to Arequipa (7750 feet) an enforced delay of a week at that city could be turned to little account, owing to the bonded truck, in which were nearly all my instruments, having been shunted off the train *en route.* Warned by this delay and the previous loss of time at Lima I utilized Mr. Thorndike's recommendation and took council with the mechanical engineers of the railway, who in a few hours transformed a 6-inch lathe into a very fair equatorial mounting by simply reversing the fast head-stock and adapting a cross axis to the face-plate. Although it was the rainy season and a good deal of rain fell, some of the nights were in part very fine, the stars shining with great steadiness and brilliancy, but its great liability to earthquake shocks however would make Arequipa an undesirable site for some classes of astronomical observations. A summary of a few thermometer readings will be found on p. 218.

Vincocaya, the loftiest station on the line was reached on February 9, but, just as I had been led to expect, the weather there was in a most hopeless state of chronic thunderstorm and downpour of rain, I therefore decided to leave the telescope and other heavy apparatus behind and push on by way of Puno to Bolivia across the Lake of Titicaca (12505 feet) with a view to gaining experience of the means of transport and the state of the sky in that country. I entered La Paz, the effective capital of Bolivia (Sucre is so only by name), on February 12. Deferring for the present an examination of the temperatures and approximate pressures recorded by me at this city and other places, I shall here endeavour to convey a general idea of the weather experienced at each of the stations successively visited.

La Paz.—The fourteen days spent wholly or in part at La Paz until February 25 fell in the rainy season, which in the year 1883 was said to be a peculiarly wet and cloudy one. This was proved by the steady rise of Lake Titicaca considerably above the average level attained of late years. Rain, generally in considerable quantities was recorded on 9 days out of the 14; thunder and lightning on five days.

I have no record of the sky having been perfectly free from cloud. The degree of cloudiness will be best seen from the annexed tabular statement—

	Clear.										Overcast.
Amount of Cloud,	0	1	2	3	4	5	6	7	8	9	10
No. of times noted,	0	1	4	8	8	15	4	9	9	11	26

The appearance of mock suns on the 20th followed by mock moons in the evening and a great prismatic halo on the next day, prove that visible haze existed sufficiently high to be frozen into ice-crystals. On the night of the 21st the sky is noted as having been very clear between the clouds.* The black bulb thermometer ranged from 122°·5 to 127°·7 without the clouds ever permitting it to rise to the full height due to continued insolation.

Vincocaya.—First visit, March 1 to 16, 1883. According to general report the usual date for the cessation of the rains had now arrived, so I resolved to return to Vincocaya (14360 feet) and there erect the telescope at a venture. The weather was very unfavourable. There were thunderstorms on ten days, and rain, hail or snow, singly or in combination on twelve of the sixteen whole days comprised in the visit. The thunderstorms were of the most awful character. At the railway station, where I passed most of my time, the highly charged state of the air made itself very perceptible, for although all the wrought-iron telegraph poles near the station were guarded by protecting wires and the usual lightning conductor forming part of the transmitting apparatus, the said apparatus during the storms was kept in a continuous state of electrical excitement. On several occasions the operator received a violent shock while prematurely endeavouring to restore communication at the close of a storm. The thunder clouds generally formed before noon, thus intercepting the sun's rays and causing the maximum temperature of the air to occur but shortly after 11 a.m. The electrical storms burst usually between one p.m. and half past four, pouring down heavy showers of rain and hail which, as a rule, changed gradually into a steady fall of snow as night came on. In spite of the very bad weather, the extreme clearness and intense blueness of the sky in the occasional gaps between the clouds combined with the good character of the definition on the few occasions on which a glimpse was snatched through the telescope, encouraged the hope that a good deal might be done at this exceptionally situated station in favourable weather. The recorded amount of clouds in the accompanying abstract of the register speaks for itself—

	Clear.										Overcast.
Amount of Cloud,	0	1	2	3	4	5	6	7	8	9	10
No. of times noted,	0	0	0	2	1	7	0	7	16	24	73

As a matter of fact I never got even a glimpse of the moon by day or night. The telescope was erected at the railway station on the lathe-bed in a small corrugated iron tool-house from which the roof was removed. The instrument was protected from the weather by light tarpaulin sacks.

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* See p. 66.—The measures of the Ilimani there mentioned gave 21220 feet as the elevation of the southern peak, the most northern one being 180 feet lower.

The black bulb thermometer attained temperatures varying from 103°·0 to 144° 9 under a partially clear sky.

Puno.[*]—March 20 to June 2, 1883. Telegrams from Puno reporting that the weather there was somewhat better than at Vincocaya, I proceeded thither on March 17, taking the telescope and other appliances with me.

During the first fortnight the weather was very wet and cloudy, but on April 6 a very marked change for the better took place. Up to that date the sky had never been altogether free from clouds, so the eleven clear occasions registered in the first period of the following table are to be credited to April 6–14 inclusive :

Table showing the Amount of Cloud at Puno.

Amount of Cloud,	Clear 0	1	2	3	4	5	6	7	8	9	Overcast 10	Rain Days.	Thunder Days.	No. of Days.	Period.
No. of times noted,	11	17	16	19	24	16	16	10	19	25	27	9	5	24[†]	I., Mar.20–Apr.14
”	35[‡]	36	24	23	23	10	12	8	10	13	7	3	3	24	II., Apr. 15–May 8
”	46[‡]	21	8	18	12	13	14	10	22	17	17	3	0	25	III., May 9–June 2

From these figures it is evident that, besides a considerable amount of perfectly clear sky, there was on many occasions so little cloud that it could only very slightly interfere with the generality of astronomical observations. To record the relative transparency of the atmosphere, which was assumed to be in proportion to the blueness of the sky, a fairly blue sky was designated by *b* ; *b* 2 indicated what would be called a very clear sky in England ; *b* 3 represented an extreme depth of clearness such as might be encountered in ordinary travel, while *b* 4 was reserved for such intense blue-blackness as I believe is only to be met with at considerable elevations above the level of the sea. The numbers of days on which *b* 3 or *b* 4 were recorded in the three periods at Puno are respectively 10, 18, and 17 ; of these again 3, 13, and 5 came more or less up to the standard of *b* 4. I say more or less, because of the 5 days in the last group, 3 are marked *b* 3–4 to indicate that there was a slight falling off in the intense limpidity of the sky which had prevailed so often in period II. However, May 29 is recorded as " a glorious day, quite clear, blue sky down to the apparent horizon [which, for a small extent to the eastward, is below the true horizon]: intense blue down to 15° altitude." Respecting this slight deterioration in atmospheric transparency towards the end of my stay, it ought to be stated that those residents in Puno whose opinions respecting the weather and other local phenomena I had found most trustworthy, promised that after a few falls of snow, to be expected in June, the air would again be as fine as anything that I had seen, and would continue so until nearly Christmas. On May 26th a strong wind, accompanied by haze, filled the atmosphere with dust, which possibly did not altogether subside until my return to Vincocaya. This dust-storm however was never so dense as entirely to obscure the two striking capes which form the "Narrows" or mouth of the outer bay of Puno, some thirteen miles from the port. One other point ought not to be passed over here: as stated in another section of this paper (p. 201) Puno becomes shaded by the steep edge of the high Pampas about an hour before sunset. When the direct sunlight ceased, no matter however clear the day had been, or however dark the colour of the sky, the heavens immediately assumed somewhat of a milky look that conveyed the idea of a want of transparency, an impression that was not dispelled even by the visibility of Jupiter and Sirius before true sunset. As the shadow of the earth rose into the air, provided the sky was otherwise serene, all traces of this whiteness disappeared.

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[*] The first floor of the Hotel Inca was found to be 12608 feet above the sea, or 103 feet above the level of Lake Titicaca as adopted by the railway survey.

[†] March 25 and 26 are not counted as I was away from Puno ; both days, however, were showery and cloudy on the lake.

[‡] These numbers include many occasions when a very small quantity of cloud rested on the lake-horizon to the eastward. As soon as it was noticed that these clouds were quite local and did not affect the rest of the sky, they were separately recorded.

Through difficulties raised by the Chilian authorities the solar spectroscope and the small equatoreal mounting did not reach Puno until June 2.

Vincocaya.—Second visit, June 5–27, 1883. During the 23 days or parts of days comprised in the second stay at Vincocaya the weather was usually extremely fine. No rain whatever fell nor was the sky ever completely overcast. Unfortunately the direct statements as to the number of times when the sky was quite clear are very incomplete, but the distribution of 47 records of the quantity of cloud as shown below may not be without interest.

	Clear										Overcast.
Amount of Cloud,	0	1	2	3	4	5	6	7	8	9	10
No. of times noted,	18	2	4	5	3	3	1	2	4	5	0

June 20 and 21 were very hazy and on the 26th a strong wind from the south-west raised innumerable whirlwinds, each with its pillar of dust, which would have rendered it impossible to observe the sun even had I not been dismounting the telescope, an operation effected with some little difficulty owing to the violence of some of the gusts. The 23rd, as stated elsewhere, was singularly fine, as also the night of the 26th, when a sketch of the Milky Way was revised.* As a rule the mornings were superbly fine and clear, but towards noon, and sometimes as early as 10 a.m., puffs of wind of a rotatory character began to come from the south-west,

* Pending the publication of this sketch, the work of many of the clearest hours at Puno and Vincocaya, it may not be uninteresting to state that it shows a complete outlying branch on the preceding side of the Milky Way in Ophiuchus, which passing in a stream about 5° in width just outside of ζ, 23, 30 and 41 of that constellation, forms a complete connexion between the diffuse southern extension in Lupus and Scorpio and the well-known projection involving β and γ Ophiuchi.

strengthening and steadying towards evening into a breeze of 4 or 5 (on a scale of 10) which eventually fell to a dead calm about 7^h or 8^h p.m.

Throughout the day in the sunshine, particularly in the afternoon on the sunny side of the station, where I lived, the temperature was most agreeable—not to say luxurious—although the thermometer stood little above 40° in the shade. It was then extremely pleasant to throw open the doors and windows or even to sit in the open air. But as soon as the sun had set, the strong wind and the falling temperature made themselves acutely and doubly felt so that even by seven o'clock the cold seemed almost intolerable. Afterwards when it became calm, the cold, although really far greater, was not nearly so inconvenient. Respecting the diminished pressure of the air consequent on an elevation of upwards of 14000 feet, it may be mentioned that those accustomed to it, enjoy the most perfect and robust health. At Vincocaya is a large machine-shop which although standing idle when I saw it, is supplied with the usual heavy sledge-hammers and other tools which were wielded with effect by the workmen in times of peace. My own impression is that it takes a grown up person some months to get used to the thin air. In my own case the sense of oppression in the breathing could always be lessened by taking several very deep inspirations. To facilitate this, the clothing should be light and loose, although exceedingly warm for the night work. Probably two distinct styles of dress will be advisable for the astronomer who settles himself in those regions—one for the day, the other for the night.

I bade a final adieu to Vincocaya at 8 p.m. on June 28th, arriving at Arequipa on the following morning, where I put up the telescope at the railway-station (7550 feet) although it had to be again dismounted and packed for the homeward voyage on June 30th. The weather was so transcendantly fine that the direct rays of the sun sent up the mercury in the black-bulb thermometer to the very top of the tube, or 205°·5 F., with a celerity for which I was not prepared at this lower level. The shade temperature however was little below 70° and thus contributed to this striking effect. The definition of the sunspots was exceedingly good, and in the opinion of Father Nardini, a resident in Arequipa, who had studied astronomy in Rome under Secchi, surpassed anything he had ever seen at the Collegio Romano with a much larger instrument.

On the first of July I went down to the sea-level at Mollendo by a special train arranged to meet a steamer for the south, which had obtained permission to pass the blockade. This steamer touched at Arica, whence I proceeded to Tacna (1840 feet) and there passed the intervening week until the arrival of the northward bound steamer from Valparaiso. The sporadic meteorological notes made at Mollendo and Tacna will be found on pp. 217 and 218.

Having thus indicated the ground traversed and the meteorological conditions experienced, I proceed to give a fuller account of such observations as I succeeded in making.

General Observations.

As already stated I arrived at Puno, for what was destined to be a long visit, on the evening of March 17, where through the kindness of the station-master a corrugated iron hut, not unlike the one already used at Vincocaya was allotted to me within the station inclosure, 12540 feet above the sea. By removing the roof of the hut and giving the lathe-bed a suitable inclination on some heavy iron castings, the telescope was put into fair working order by the 19th. That night the weather partly cleared up and I got a view of several objects. The definition was not particularly good, but still γ Centauri was well divided. Several poor nights succeeding, I made an excursion to the Island of Titicaca from March 24 to 27. On the morning of the 31st came a fine interval of sunshine in which the solar radiation thermometer (black-bulb) rose to 201°·5 and would have risen still higher had the tube been long enough to warrant further exposure to the sun's rays. With a power of 163 and an unsilvered diagonal plane a large sun-spot was exquisitely defined, exhibiting a great amount of detail in the luminous tongues which the penumbra extended into the umbra. After a thunderstorm the night was partly clear, the Milky Way coming out magnificently, but Antares which was low down scintillated violently, and even Arcturus and other high stars twinkled perceptibly. The following evening, that of Low Sunday, the unsettled weather culminated in a tremendous thunderstorm, which I hoped would clear the air. The stars indeed came out steadily about 11 p.m., but the sky was still hazy. On April 3, Jupiter was found shortly after noon by setting in declination in the meridian by a small clinometer and using the main wheel[*] of the lathe as an hour circle. The belts were visible as a reddish brown band, but I could not find the satellites. At 2 p.m. the black-bulb stood at 200°·5 : clouds came on at $2^h\ 30^m$ p.m. At night I was able to examine the stars with the higher powers of the telescope (362 and 401 diameters). In so doing I found β Muscæ to be beautifully double : estimated position and distance, 280° and 0″·8, 5 and 6 mags., both stars full yellow. On April 6 the definition was again good : without a knowledge of earlier observations I recognised at a glance the duplicity of *h* 4165, which has a distance of 1″·4 in the C.G.H. observations. H Velorum also turned out to be double[†] with an estimated distance of 1″. Northern stars in Cancer and Leo gave clean circular images excepting Regulus which did not seem to be quite round, possibly owing to some defect of the glass brought out by the brightness of the star. Saturn at an altitude of 25° was fairly well defined, while Jupiter then 45° above the horizon was well seen with a power of 362.

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[*] This wheel had the rather awkward number of 63 teeth, but by increasing the hour angle by $\frac{1}{20}$th part, each tooth of the wheel corresponded to 24 minutes of this augmented time and admitted of easy subdivision.

[†] I find that both β Muscæ and H Velorum had been previously recognised as double stars by Mr. Russell using the 11½ in. Schröder telescope, at Sydney. See *Results of Double Star Measures, Sydney*, 1882, 8vo, p. 62, where it must be noted that the name of the former star is misplaced. Mr. R. measured the distance of β Muscæ at 0″·54, 1878·284 adding the note; "one of the closest doubles I know."

On April 10 the definition is recorded as being almost perfect and on that night several very close double stars were detected which are believed to be *novæ.* With power 400 the stars exhibited clean round disks surrounded by delicate diffraction rings, but the definition after all was not absolutely perfect, for although the stand was quite free from tremor the images moved with a certain degree of irregularity across the field of view while the diffraction rings twisted or changed in outline to a perceptible extent. The succeeding nights varied very much in quality although hardly so much as at the level of the sea, still occasions were not wanting on which the definition was very bad indeed. The small table of double stars detected or examined will however give the surest criterion of what might be seen with a 6-inch glass under fairly favourable conditions at a height of 12540 feet within the tropics.

List of Double Stars.

No.	R.A. 1883·0.	Decl. 1883·0.	Remarks.	Date, 1883.
1	12^{h} 39^{m} 7^{s}	— 67° 28′·0	β Muscæ, 280°, 0″·8; both full yellow, 5m. and 6m. Stone 7053. Is Russell 207.	April 3.
2	8 58 7	— 51 43·7	Stone 4816, *np*[*], 2″, 6½ and 8 mags.; is h 4165, 88°·2, 1·41″. 6m. and 8m., 1837·24.	April 6.
3	8 52 47	— 52 16·5	H Velorum = Stone 4757; *nf*, 1″; 6·5 and 7·2 mags. Is Russell 87.	April 6.
4	9 26 6	— 39 57·3	ψ Argûs = Stone 5124; 45°, 0″·8; 4 and 6 mags.; both white; *nova.*	April 10.
5	10 41 44	— 48 48·1	μ Argûs = Stone 5957; 70°, 2″; 3 and 8 mags.; white and sea green Is Russell 155.	April 10.
6	12 35 4	— 48 19·0	γ Centauri = Stone 7022; 170°, 2″; both 4m.; is h 4539, 354°·3, 0″·75; 4 and 4; 1835·9.	April 10.
7	15 3 48	— 48 17·5	κ Lupi = Stone 8251; 90°, 1″; both yellow; h has a 7 mag. companion at 142°·8, 25″, while the *Melbourne Catalogue* notes a companion 6 mag. *sp*[†]; *Nova?; unless π Lupi was examined by mistake.*	April 10.
8	15 10 24	— 47 26·6	μ_1 Lupi = Stone 8307; 1″·5. = h 4753, 2″·08, 1836·83.	April 10.
9	15 14 44	— 44 16·0	ϵ Lupi, 285°, 0″·8; 4m. and 7m.; white and ashy; *nova.* Sir J. Herschel and Dunlop note a distant companion.	April 10.
10	15 27 50	— 44 33·9	*f* Lupi = Stone 8468; 0°, 3″; white and greenish. Is h 4788, 349°, 1836·62; 3″·14, 1836·42; 5 and 9 mags.	April 10.
11	11 1 52	— 42 0·4	Stone 6169; 270°, 1½″, 6 and 7·5 mags,[‡] . .	April 24.
12	18 9 43	— 36 47·7	η Sagittarii = Stone 9962, *f*, 4″, 3 and 9 mags. Large star, full orange. [§]	April 18.
13	18 58 30	— 37 13·8	γ Cor. Aust., Stone 10373. *nf*, 1½″, both 6 mag. and both full yellow. [‖]	April 18.

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[*] No. 2. Either I have made a mistake in the quadrant or this pair is in rapid orbital motion.

[†] No. 7. This is probably a misprint for *sf*, the companion being the 6^{m} star Stone 8252 which follows 1^{s}·6, 22″ to the south, and is identical with h's star.

[‡] Measured by h with the 5-inch equatorial, 278°·0, 1″·40, 1837·25.

[§] Is Birmingham 432, Schmidt, Ast. Nach. 1902, deep golden yellow.—Gould *r*. The companion was discovered by Burnham on Mount Hamilon in 1879, who estimated the companion at 10·5 to 12·0 mag.

[§] Noted "close double" in the great Cape Catalogue. Is h 5084. Burnham measured 240°·0, 0″·87, 1879·695.

On several occasions at Puno I noticed what seemed to be a beautiful pair of about the 12th magnitude between the two chief stars, A and B, and the third star, C, of α Crucis. Their position angle does not differ greatly from that of A and B but still I believe this to be an actual double star and not merely a reflex of the large pair. I hoped to settle the point decisively at Vincocaya, but by that time the position of α Crucis had become less favourable and I did not succeed in satisfactorily seeing the small double.

Respecting the clearness of the air it is very difficult to convey an exact impression of the actual gain achieved at a particular station. I have already mentioned the appearance of Jupiter in the telescope in the daytime and the fact that his satellites were invisible. Had the telescope been properly mounted it would have been easy to set on a number of stars of various magnitudes and decided down to what degree of faintness they could readily be made out. This course failing for want of setting circles, I endeavoured to determine how far the brighter stars or planets might be visible to the naked eye in the daytime or very shortly after sunset. First I tried Sirius near the zenith. This was the more readily done as Sirius culminates within about 43′ of the zenith at Puno. Guided only by a rough idea of the time of the star's meridian passage I tried on several days to pick up the star as it passed behind the projecting eaves of a roof vertically above me, but in vain. This was in the beginning of April when Sirius was 3 to 4 hours following the sun. The next plan was to find if Sirius became visible before sunset at all. On the 21st of April standing in the shade in the courtyard of a friend's house I succeeded in picking up Sirius at a time that turned out to be $9^m 20^s$ before geometrical sunset, or the moment when the computed zenith distance of the sun's centre was 90°. A couple of minutes later both my friend and his wife also saw the star distinctly. It is necessary to mention that Puno and particularly the part in which I lived is shaded for some time before sunset by hills to the westward. In the following table the computed time of sunset is that defined above. In comparing these observations with others of the visibility of stars after sunset made at sea it should be noted that in these latter the time of sunset is generally taken to be either the moment when the sun actually disappears below the apparent horizon, or when his disk is half submerged.

Visibility of Fixed Stars and Jupiter with the naked eye near the time of Sunset, at Puno.

Date, 1883.	Object.	When seen. Chron. time.	Chron. corr.	M. T. of geom. sunset.	Time from sunset.	Remarks.
Apr. 21,	Sirius, .	$5^h\ 22^m\ 45^s$	$+12^m\ 41^s$	$5^h\ 44^m\ 46^s$	$9^m\ 20^s$ bef.	
,, 23,	Sirius, .	5 13 45	+ 12 25	5 43 37	17 27 bef.	The star was first found by the eye, guided by the finder of the telescope; but immediately afterwards it was readily seen without such guidance. The sun was shining brightly on the neighbouring hills and the islands in the lake of Titicaca.
,, 25,	Sirius, .	5 27 0	+ 12 9	5 42 7	2 58 bef.	
	Jupiter, .	5 34 0			4 2 aft.	Jupiter bright.
	Canopus,	5 39 0			9 2 ,,	
	β Orionis,	5 46 30			16 32 ,,	
	*Orionis,	5 52 0			22 2 ,,	1st star in Orion's belt.
	—	5 52 30			22 32 ,,	All three stars in the belt.
,, 26,	Jupiter, .	5 16 0	+ 12 1	5 41 53	13 52 bef.	Through haze [or whiteness of the sky].
,, 29,	Sirius, .	5 20 30	+ 11 37	5 40 16	8 9 bef.	
	Jupiter, .	5 21 12			7 27 ,,	
	Canopus,	5 36 30			7 51 aft.	Star exceedingly bright.
,, 30,	Jupiter, .	5 6 10	+ 11 29	5 39 45	22 6 bef.	Planet fairly distinct.
	Sirius, .	5 8 0			20 16 ,,	Bright.
	Canopus,	5 31 5			2 49 aft.	Star very bright.
May 12,	Jupiter, .	5 31 40	+ 10 6	5 34 43	7 3 aft.	Excessively bright.
	Sirius, .	5 35 30			10 53 ,,	Very bright.
	Canopus,	5 36 30			11 53 ,,	Very bright.
	Procyon,	5 42 0			17 23 ,,	Excessively bright.
,, 15,	Jupiter, .	5 20 20	+ 9 40	5 33 47	3 47 bef.	
	Sirius, .	5 21 50			2 17 ,,	
	Canopus,	5 23 15			0 52 ,,	
	Procyon,	5 35 35			11 28 aft.	Fairly bright, sky rather white.

The observations of May 12 are to be considered as a failure. The stars, since last observed, had changed their positions relatively to the neighbouring roofs, &c., as seen from my standpoint, and my attention was consequently not directed to the proper places. The record serves to show how easily the stars might be overlooked even when searched for.

In the second chapter of the third volume of *Cosmos* (pp. 72 and 115), Humboldt has gone thoroughly into the historical side of the question of the visibility of stars in the daytime, quoting Aristotle who was of opinion they were so seen from deep vaults or cisterns, an opinion that is repeated by Pliny. Humboldt himself however, who as inspector of mines had the greatest facilities for making the experiment and frequently contemplated the vault of the heavens from deep shafts, was never able to discern a single star. Nor in the mines of Peru, Mexico, or Siberia did he ever succeed in finding an individual who had heard of seeing stars in the daytime. On the other hand he cites the interesting passage from §61 of the *Outlines of Astronomy* where Sir John Herschel states that a celebrated optician had seen at a certain hour, on several successive days, a considerable star through the shaft of a chimney. Humboldt also gives the following passage by Saussure as referring to observations at an altitude of 12757 feet: "Quelques-uns des guides m'ont assure, avoir vu des étoiles en plein jour; pour *moi je* n'y songeois pas, en sorte que je n'ai point été le témoin de ce phénomène; *mais l'assertion uniforme des guides ne me laisse aucun doute sur la réalité.* Il faut d'ailleurs être entièrement à l'ombre, et avoir même au-dessus de la tête une masse d'ombre d'une épaisseur considérable, sans quoi l'air trop fortement éclairé fait évanouir la foible clarté des étoiles." Again in opposition to this account he gives the negative result of the brothers Schlagintweit who traversed the Tyrol to altitudes of 13016 feet but neither saw the stars by day themselves nor found amongst the shepherds or chamois hunters any report of their being thus visible. But strangely enough Humboldt passes over quite briefly his own experience of repeatedly and easily seeing Jupiter with the unaided eye in the ethereally pure sky of Cumana at the level of the ocean after observations of the satellites of Jupiter, when the sun stood already 18° to 20° above the horizon.

I have given the above notes at length because they cover the whole ground as it were. Respecting the visibility of stars in the daytime from the interior of vaults, cisterns or mines or through tall chimneys, the small area of the portion of sky so visible and its restriction to one parallel of declination for any given place greatly lessen the chances of accidentally picking up a bright star. To these limitations may be added the inconvenience of the observer's attitude, unless indeed he laid down on his back, and—in the case of a mine—the difficulty of finding a shaft sufficiently free from mechanical hindrances and the risk of falling stones. Assuming the young optician to have observed in England he must either have detected some second or third magnitude star in bright twilight, or have seen some more brilliant object, which may have been Jupiter or Venus, through an inclined flue, in either case the sooty blackness of the passage greatly contributing to the result.

The evidence of Saussure's guides indicates as essential conditions exactly those under which I saw Jupiter, Sirius and Canopus at Puno, thus rendering it practically certain that their statements were based on actual experience. It would therefore not be without interest if some zealous Alpine tourist were to make a trial of what can be seen under favourable circumstances. The simplest and easiest plan would probably be to secure the necessary allignments in the morning twilight and endeavour to follow the stars into the daylight. In this way, watching from the deck of a ship in the tropics about the end of August 1858, I continued to see Jupiter and even Sirius until the sun showed above the horizon, but after glancing at the sun I was unable to again find Sirius although I succeeded in picking up Jupiter for a short time.

I here add two notes as to the appearance of the Moon to the naked eye in the daytime. The first refers to $9^h\ 8^m$ a.m. on June 2, at Puno. The Moon, 2 days 16½ hours before change, was a conspicuous object, recognised at the first glance in the blue-black sky in spite of a few faint cirri. Its angular distance from the sun was 28°, and the greatest breadth of the crescent $1'{\cdot}9$. The second occasion was at 4.30 p.m. at Vincocaya on June 11. The Moon at this time was high in the sky, and somewhat short of the first quarter. Its appearance was very like that in photographs, the *Mare Serenitatis* being nearly black.

The first of these memoranda is not given as by any means defining the least distance from the Sun at which the Moon might have been seen, but rather as showing its extreme visibility under the circumstances described, and also as a reminder to future observers. At p. 151 of "Der Mond," Mädler gives a very interesting account of an observation of the Moon with a telescope only 18 hours before New Moon.

Spectroscopic Observations.

(a.) With the Vogel Spectroscope.

Having amongst the apparatus at my disposal, the small spectroscope, fully described on page 105 of the first volume of this journal, I naturally took an early opportunity of attaching it to the telescope and examining the spectra of some of the brighter objects in the southern heavens. To evade the necessity of using any eye-lens, either cylindrical or spherical, I had before leaving Dun Echt inserted a lens of about 2·8-inch focus between the scale and the prism so that the scale divisions were plainly visible with the eye close to the spectroscope which thus became reduced to a prism, collimating-lens and slit. By removing the latter any object sufficiently small gave a linear spectrum, and the instrument became practically reduced to Zöllner's arrangement of a direct-vision prism between the eye and the eye-piece; the eye-piece in this case consisting of a single lens. My rashness in trusting to the library at Quito for aid in the way of books caused me to be without a map of the great Nebula in Argo, or, indeed, any map of the southern heavens beyond the small one in Argelander's Uranometria Nova, and the spectroscope failed to distinguish η from the other stars in the nebula. But quite otherwise was the case of γ Argûs when on April 24th I first viewed its spectrum in the open field of the prismatic eye-piece. Its intensely bright line in the blue, and the gorgeous group of three bright lines in the yellow and orange, render its spectrum incomparably the most brilliant and striking in the whole heavens. To a great extent it was the extraordinary beauty of this spectrum (which, as I have since learned, was first seen by Respighi, in 1871) that led me to devote a considerable part of my time to more or less systematic sweeps of the neighbourhood of the Milky Way and its neighbourhood, on the plan first advocated, in this special form at least, by Professor Pickering in the October number of the *American Journal of Science* for 1880.* The results of this search are given below, and to them have been added the approximate determinations of wave-lengths of such lines as I was subsequently able to measure at Vincocaya after the receipt of the equatorial mounting and driving-clock. The places of the two first objects are from Mr. Stone's catalogue, while the others are mere approximations obtained by the somewhat damaged circles of the instrument, and may be in error to the extent of 2′ or possibly more, but anyone searching with a prism can hardly fail to identify the objects indicated.

1883, April 24. The spectrum of γ Argûs consists of three intensely bright and strong lines and one slender one on the ground of a moderately bright continuous spectrum. This spectrum is far finer than that of γ Cassiopeiæ.

Spectrum.	June 21.	June 24.	Mean.
	mmm.	mmm.	mmm.
Begins,	680±	670±	675±
Orange line,	583·2	578·6	580·9
Citron „	566·2	567·3	566·8
Blue „	465·2	464·1	464·6
Ends,	435±	405±	420±

Assuming the mean results to be correct, the fourth and thinnest line seen in April must be about W.L. 590 mmm. $\alpha = 8^h\ 5^m\ 56^s$, $\delta = -46°\ 59'·5$ for 1883·0.

April 30. All the stars in the great cluster G.C. 4245 near ζ Scorpii have ordinary spectra except the 7·5 mag. star P. XVI. 204 = Stone 9168, which has a bright line in the blue and two in the yellow very like those in γ Argûs. $\alpha = 16^h\ 46^m\ 7^s$, $\delta = -41°\ 37'·5$.

Spectrum.	June 21.	June 24.	Mean.
	mmm.	mmm.	mmm.
Begins,	640	. . .	. . .
Line,	582·0	579·8	580·9
„	569·8	568·6	569·2
„	465·8	464·4	465·1

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* As a means of examining known nebulæ and clusters, the open prismatic field was used by Captain J. Herschel, at Bengalore in 1868. See *Proc. Roy. Soc.*, Vol. XVII., pp. 303–307, where in particular the planetary nebula G. C. 1565 is thus described: "It is . . . . situated in a magnificent cluster of stellar points. In the open field of the spectroscope it appeared as a . . . . faint patch of light, in the midst of an infinity of streaks." In touching on the history of this spectroscopic method, it is interesting to call to mind the experiments of Thomas Melvill, as described in a paper read in Edinburgh, on January 3 and February 7, 1752. Speaking of the flame of alcohol mixed with sea-salt, he says:—"The proportion in which the bright yellow exceeds the other colours in this light is still more extraordinary than in the former; insomuch that the hole seen through the prism appears uniformly of this yellow, and as distinctly terminated as through a plain glass, except that there is adjoining to it on the upper side a very faint stream of green and blue. White bodies illuminated by it appear also through the prism perfectly well defined; both which are very surprising phenomena to those who have been accustomed to the use of the prism in other heterogeneous lights, where it never fails to throw confusion on the extremities of all visible bodies."—See a letter by the late Professor Jevons, *Chemical News*, Vol. V., p. 251. The priority of utilizing the open field of a tele-spectroscope formed by a prism in front of a telescope of 13 lines aperture, with the addition of a cylindrical lens to give breadth to the otherwise linear spectra of star-like points, is due to Fraunhofer, as shown by the following passage from p. 43 of his *Détermination du pouvoir réfringent*, *&c.*:—"J'employai ce même appareil pour regarder dans la nuit la planète Vénus, *sans laisser tomber la lumière par une petite ouverture*. Je trouvai dans le spectre de cette lumière les mêmes raies, telles qu'on les voit dans celui de la lumière du soleil." The italics are in the original and show the stress Fraunhofer laid on the capabilities of this method.

1883.

May 2.—Found a planetary nebula equal in brightness to a 9·5 magnitude star. With a power of 180 it is still quite stellar. There is an $11\frac{1}{2}$ mag. star *sp*, 3″ distant. A 10 mag. star precedes $8^s\cdot8$ by the chronometer, about 10″ south. Two other stars complete a parallelogram. The nebula is quite monochromatic. $\alpha = 15^h\ 5^m\ 14^s$, $\delta = -37°\ 39'$.

May 4.—Found another small planetary nebula resembling a star of $10\frac{1}{2}$ magnitude. A star of 7·5 mag. precedes by about 35^s and another 11·5 mag. by 22^s. At Vincocaya, on June 9, I detected a 13m. star about 50″ off, in position angle 310° or thereabouts. α, $13^h\ 45^m\ 43^s$; δ,— 65° 57′. On the same evening I came across a 9th magnitude star with a faint continuous spectrum and two bright lines far apart. It forms the *np* corner of an equilateral triangle with two other stars of the same size. All three are of the same yellow tint. $\alpha = 13^h\ 10^m\ 37^s$, $\delta = -57°\ 31'$.

	June 21.	June 24.	Mean.
	mmm.	mmm.	mmm.
Yellow line,	572·0	578·6	575·3
Maximum of continuous spectrum,		500	500
Blue line,	463·0	463·6	463·3

Later on the same night I met with a 9 mag. white star with a spectrum of the same type as the foregoing except that the lines were less intense in comparison with the continuous spectrum. I subsequently found its approximate place to be : α, $11^h\ 5^m\ 19^s$; δ, — 60° 21′.

May 7.—Found an 8·8m. star with a faint continuous spectrum slightly stronger in the middle, with one fairly bright yellow line and an intense blue one. There is a 13 mag. star about 20″ to the north, and a 6·5m. star 67^s preceding. There are many other stars in the field, the two nearest being in positions 305° and 120° by diagram. The preceding star is Lac. 3,596, which is 5·8m. in the Uranometria Argentina. Positions of lines, June 24, 573 and 463·6 mmm., α, $8^h\ 51^m\ 1^s$; δ, — 47° 8′. On the same evening I encountered yet another star with the same style of spectrum with two bright lines. It is of the 9·8 magnitude, and lies to the north of the Great Nebula in Argo, in the same field with two red stars, Lac. 4401, which I estimated of the 7th mag., and Lac. 4435 of the 6th magnitude. Gould has the stars "6·8 *r*" and "6·5 *r* var. ?" α, $10^h\ 36^m\ 54$; δ — 58° 8′.

May 24.—A planetary nebula of about $9\frac{1}{2}$ magnitude was seen first on this night, but thunder clouds advancing across Lake Titicaca prevented further observations.

May 27.—The planetary nebula is the southernmost object in a slightly elongated lozenge of which the *f* star is red and of the 8·5 mag. Length of the lozenge $pf = 20^s\cdot8$. The nebula is not quite stellar in the spectroscope. α, $16^h\ 23^m\ 28^s$; δ, — 39° 59′.

In the course of these sweeps I also encountered four stars whose spectra were so strongly banded as immediately to challenge attention. The two first are certainly of Secchi's fourth type, with three principal bright bands sharply bounded towards the violet. Two of them had already been noticed by Sir John Herschel on account of their deep red colour. Their places and dates of observation are :

May 12.—8m. star with a very beautiful spectrum containing three bright bands, of which that in the yellow is the most intense, rising almost to a bright line. Type IV. Fine red orange colour, 4 small stars near, two to the north and two to the south. α, $12^h\ 8^m\ 16^s$; δ, — 50° 54′.

May 13.—Red star of the fourth type, 8·2 mag. α, $13^h\ 50^m\ 0^s$; δ, — 55° 46′.

Same date.—Found a star of 8·2 mag. with a finely banded spectrum, intensified in parts almost to bright lines. The somewhat uncertain place is $14^h\ 8^m\ 5^s$, — 59° 17′, hence the star is identical with Gould's R Centauri which he found variable from 6 to 10 mag. It is Schjellerup 166, and was described by Sir J. Herschel as *ruby or high orange.* It is interesting that it should have been detected afresh with the prism.

May 27.—9 mag. star, full red, with a spectrum of three bright bands. It is Schjellerup 172, and was noted as "ruby coloured" by Sir John Herschel.

Amongst the objects described in the foregoing observations, including γ Argûs, are six stars with bright lines in their spectra. As far as my measures and estimates go, all of them belong to the same class as the three Wolf-Rayet stars in the Swan, to which group Prof. Pickering has since added a fourth outlying member. Of at least a closely allied character are also Lalande 13412 and Argelander-Oeltzen 17681 whose spectral peculiarities were also detected by Prof. Pickering ; see this journal, Vol. I., pp. 86 and 140. For Prof. H.C. Vogel's recent spectroscopic observations with the great Vienna equatorial of the last-named stars and those found by Messrs. Wolf and Rayet see Sitzb. der K. Akad. der Wissensch. II. Abth. Oct. 1883, Vol. LXXXVIII.

I here append a few stray observations of the spectra or magnitudes of some southern stars, giving the dates whenever they seem at all likely to be of use. The numbers of the Uranometria Argentina and a few remarks from that catalogue and notes from other sources are added.

List of Stars whose Spectra were casually Observed at Puno.

Name and No. in U. A.	α, 1880.	δ, 1880.	Remarks on the Spectra, &c.
α Arg. Carinæ, 7, .	6^{h} 21^{m} 13^{s}	— 34° 59′·7	Type I., *F* line very strong.
o^{1} Canis maj., 92, .	6 49 9	— 24 2·1	Slightly banded. Deep orange red, 4·5 mag., May 2. The U. A. has 3·9 mag. *r* [red].
σ Canis maj., 114, .	6 56 56	— 27 45·8	Slightly banded. Very deep yellow, 4·2 mag., May 2. 3·6 mag. *rr var.*? in the U. A.—Pechüle,* type II.
π Puppis, 82, . .	7 12 54	— 36 53·0	Type III., beautifully banded.—Pechüle, type II. Pickering,† "bands?".
c Puppis, 175, . .	7 40 59	— 37 40·7	Type III., full orange red, 5 mag., April 10. The U. A. has 3·6 to 3·9 mag., for various observers ; colour *a deep orange ;* neighbouring stars embarrass the estimates of brightness. Pickering, bands?
χ Carinæ, 65, . .	7 53 44	— 52 39·7	Type III.
ε Carinæ, 89, . .	8 20 3	— 59 7·4	Type II., feebly banded.
δ Velorum, 65, .	8 41 23	— 54 16·2	Type I., *Ha*, β, γ, and δ as black lines.
α Volantis, 46, .	9 0 33	— 65 55·0	Type I., *Ha*, β, and γ.
λ Velorum, 100, .	9 3 35	— 42 56·9	Type III., fine banded spectrum shaded to the red.—Pechüle, type II. with two zones.
κ Velorum, 129, .	9 18 24	— 54 29·9	Type II.
N Velorum, 144, .	9 27 35	— 56 30·3	Type III.?, banded spectrum.—Pechüle, type II. with two zones.
γ Crucis, 34, . .	12 24 31	— 56 26·5	Type III., a most magnificent spectrum ; see below.
α Centauri, 363–4, .	14 31 28	— 60 20·3	Type I. or II., *F* and *b* conspicuous.
ϕ^{1} Lupi, 88, . .	15 14 12	— 35 49·5	Very deep orange, 5 mag., April 11. In the U. A. it is *rr*, 3·6 mag. The difference of these estimates is suggestive of variability.
ω Lupi, 117, . .	15 29 58	— 42 10·3	Very deep orange. *rr* in the U. A.
α Scorpii, 69, . .	16 22 3	— 26 9·8	Type III., has bands very like those of γ Crucis ; see below.

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* Expédition Danoise pour l'observation du Passage du Vénus, 1882. Copenhague, 1883, 8vo. It contains spectroscopical observations of many southern stars.

† Ast. Nach. No. 2405.

The following wave-lengths have been deduced from estimates of the dark bands of the spectra of Antares and γ Crucis on June 24, 1883. All the bands are darkly shaded towards the red, and closely resemble those of α Orionis and α Herculis.

	Antares.	γ Crucis.
	mmm.	mmm.
Begins,	700 ±	700 ±
	653·8	655·8
	621·6	618·6
	589·3	585·5
	567·5	564·2
	550·5	550·5
A line (E?) *	526·9	526·9
	521·5	. . .
	. . .	516·6
	501·7	499·0
	487·7	485·5
Ends,	433·5 ±	433 ±

Spectrum of Lightning.

During a violent thunderstorm on the evening of March 31, 1883, I observed a number of bright flashes of lightning with the Vogel spectroscope. Whenever a flash was sufficiently bright and the spectroscope favourably held a fine long spectrum was seen with bright bands or lines and also dark shaded bands. The resulting wave-lengths of the scale readings are :

Wave-length. mmm.	
670	The spectrum begins.
643	A dark band between two bright ones.
567·5	There is a series of dark bands at the red end extending as far as this wave-length.
571·8	A bright band.
565·2	"
530·6	"
498·4	"
464·4	"
436	The spectrum fades off.

Respecting the possible origin of the lines, it may be that the dark band 643 is the interval between 648·0 N and 645·2 O with what would be a slight error of observation with this spectroscope ; or it may be between these lines and 656·2 H, with a somewhat larger error. In this part of the spectrum an error of $\frac{1}{10}$th of a part in the scale, corresponds to a change in the wave-length of 1·8mmm. The lines 571·8 and 565·2 fall close to the great nitrogen lines 571·1, 568·1 and 566·6 ; 530·6 is again in close accord with a pair of nitrogen lines 533·0 and 530·9, if it is not also associated with 534·0 O and 531·5 O. 498·4 is also probably the great double line of nitrogen 500·5 and 500·2, with an admissible error. Lastly 464·4 agrees exactly with a very strong nitrogen line as determined by Plücker, whose wave-lengths I have taken throughout from Dr. Watts' *Index of Spectra.*

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* In applying a constant correction to the scale readings this line has been assumed to coincide with E, with which it closely agrees.

Spectroscopic Observations of the Violet End of the Solar Spectrum.

On the 24th of June, while taking readings of the chief solar lines with the small spectroscope so often mentioned, for the purpose of constructing a table of wave-lengths for each division of the scale, I used a piece of cobalt glass to get a better view of the *H* lines. In so doing I was surprised to see quite a number of lines beyond H_2 or *K*, and eventually succeeded in getting readings for 6 lines or bands, beyond the last of which I could still see the light, but no further lines. The upper part of the reducing curve, with the last two readings extrapolated, is shown in the following small table—

Scale reading.	Wave-length.
	mmm.
36^{p}	417·3
38	412·3
40	407·5
42	402·8
44	398·4
46	394·1
48	390·0
50	386·0
52	382·2

By the aid of this table the subjoined scale-readings have been turned into wave-lengths, to which the wave-lengths of the presumably corresponding lines in M. Cornu's chart have been appended. It will thus be seen that *L* was the furthest visible line, a fact confirmed by the absence of any very strong line immediately beyond it. Through the same piece of glass the line *A* is also visible—

Reading of Scale.*	Wave-length from Curve.	W.L. (Cornu).	
	mmm.	mmm.	
44^{p}·7	396·9	396·8	H′.
46·5	393·1	393·3	K.
47·1	392·0	392·0	
48·0	390·0	389·5 to 390·5	4 lines.
49·0	387·9	387·7 to 388·1	line and band.
50·3 or 50·05	385·3 or 385·9	385·9	strong line.
50·60	384·8	385·0	strong band.
52·1	382·1	382·0	L.

The Zodiacal Light.

Many times during the journey did I look at the spectrum of the Zodiacal Light with the hope of distinguishing some of the bright lines that have been ascribed to it. But on no occasion did I see practically more than is visible on clear, moonless evenings in January at Dun Echt. On June 6th, 1883, at $7^h\ 20^m$, local time at Vincocaya, 14360 feet above the sea, the great lenticular beam of light was splendidly visible, stretching right up to the zenith, but the small spectroscope disclosed nothing but a disappointingly short continuous spectrum. The observation was repeated under very favourable circumstances on June 24, at the same place, when the roughly approximate limits of the spectrum were laid down at wavelengths 538 and 473 mmm. This result is not very different from those of Professors Smyth* and Wright,† but there is this difference, that while Professor Wright's diagram places the maximum at W.L. 522, the Vincocaya estimate is 502, or appreciably further towards the violet. I very much regret that I had not a copy of Professor Wright's highly suggestive and interesting paper with me, or I should probably have been led to try to see the Fraunhofer lines in the spectrum. I ought to add that I made no attempt to determine the extreme limits of the spectrum but merely set down the boundaries of the feeble continuous light by the aid of the faintly and intermittently illuminated scale of the instrument. Hence the limits given are far within those set down by Professor Wright, who took the precaution of using a single prism while I used a direct-vision one. The difference in the position of the maximum may be due to one or several of three causes—1stly, to difference in the absorptive action of the prisms; 2ndly, to physiological difference of susceptibility to rays of various colours on the part of the observers; 3rdly, to increased transparency of the air at high altitudes to rays of greater refrangibility. Not improbably the third hypothesis is the most tenable and would suggest an attempt to photograph the spectrum, faint as it is. A very long exposure might be given, particularly if a similar plate were exposed for the same length of time to the spectrum afforded by a part of the sky illuminated by stars only.

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* These quantities are the mean of two sets, after the first had been reduced to the second for a displacement of the prism caused by the shrinkage of its fastenings owing to the extreme dryness of the air.

(*b.*) *Observations of the Sun with the Browning Automatic Spectroscope.*

Amongst apparatus sent to me from Dun Echt and which eventually reached me at Puno on June 2nd was one of Browning's automatic spectroscopes of the well known construction. The collimator and viewing telescopes are of $\frac{3}{4}$-inch aperture and $6\frac{1}{2}$-inch focus. On emerging from the collimator the rays are turned at right angles by a total-reflecting prism $\frac{5}{8}$ in. high and passing in succession through a half-prism and a train of prisms, one to four in number, are received by another half-prism, at the back of which a large total reflecting prism returns the rays through the upper half of the whole train, whence they pass into the viewing telescope at right angles to the collimator. The prisms of the train are all $1\frac{1}{4}$ in. high by 1 in. face. On the Dun Echt instrument there is a special graduated position circle forming part of the spectroscope and also two rectangular slides, apart from the position circle of the telescope to which it may be attached. This gives great facility in placing the slit in any angle of position on any part of the sun. Indeed by proper management the slit can be swept round the sun in any position with regard to the limb or it can be rotated round any point. Any of the prisms of the train can be removed and the terminal half-prism substituted in its place so that the effective power of the spectroscope may be 2, 4, 6, 8 or 10 prisms. Unfortunately some of the prisms were badly splintered in the rough journey across the pampas and unluckily the half-prisms (the indispensable part of the instrument) were the worst sufferers. Various injuries to the metal parts I had either repaired before leaving Puno or had patched up at Vincocaya, so that the spectroscope in this respect worked very fairly.

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* Monthly Notices, Vol. 32, p. 277, and Edinburgh Observations, Vol. XIV.

† American Journal, Ser. III., Vol. VIII., p. 39.

The points I proposed to examine were: the possibility of seeing the bright lines of the spectrum of the corona or generally in the sun's atmosphere, and an examination of the red end of the spectrum with a view to determining if the great lines in that region still retained their full intensity when seen from an elevated station. I now give such abstracts from my note-book as seem of any interest.

June 9.—Saw the first prominence with the spectroscope, 11 15 a.m., whirlwinds. Day-wind began at 11 a.m.

June 10.—10^h 10^m a.m. A solar prominence was seen in three layers on the preceding limb. The lowest layer as a thick chromosphere and then two parallel layers above this: seen in C, D_3 and F. This was seen almost as well when all four of the intermediate prisms were taken out. Made many attempts to see bright lines in the corona. Dark [Fraunhofer] lines were well seen up to about 5′ altitude fading rapidly away into darkness. Tried with various combinations of prisms; but unfortunately the first and last are splintered and doubtless introduce much false light.

Blackened the broken parts of prisms and tried at 1 p.m. By this time the wind had risen and there was a slight amount of dust in the air that sufficed to fill the whole slit with light; so that there was no chance of seeing coronal lines. Wind also troublesome.

June 11.—9 12 a.m. Solar spectrum with 3 intervening prisms. Many prominences very fine in C, D_3 and F, but not in other colours. Tried at various heights above the sun's surface. No trace of coronal lines. The nearest approach to a bright line is the space between the two close lines of *b*. [W. L. 516·7].

4 30 p.m. To redward side of C is a large band with a bright interval and then fine lines, beyond this the spectroscope fails. [This was of course B and its companion lines.]

June 15.—A fine group of prominences on the preceding limb visible in C and D_3.

June 17.—Tried all day at the red end of the solar spectrum. [A diagram made at the time shows C, B, *a* and A apparently in their full strength; the prisms had doubtless been readjusted since the 11th.]

June 19.—Spectroscope on Sun. Prominences in C, F and D_3. Tried for hours with slit radial and tangential but could get no other bright lines.

June 23.—A short hooked prominence seen. By careful focussing saw the prominences in four lines, C, D_3, F and $H\gamma$. F was reversed in one of the spots. At 2 p.m. saw five sun spots in the Sun's image as projected though a nail hole in the iron roof of the railway station. Diameter of image 2·7 inches. In the roof of the large unused machine shop I found a hole at such a distance as to give an image of 4·8 inches diameter with five spots beautifully distinct. They were nearly in a straight line, first three and then two. The hole must have been 44 feet from the paper on which the image was projected.

From these notes it will be seen that I completely failed to detect any bright lines in the spectrum of the sun's atmosphere beyond D_3, and the three Hydrogen lines α, β and γ, but on examining recent investigations on the subject I am by no means convinced that on June 10 I did not see something of the continuous spectrum of the corona. It is now known that M. Janssen saw about a hundred of Fraunhofer's lines in the coronal spectrum during the total eclipse at Caroline Island on May 6, 1883.[*] The planets Venus and Mercury under favourable circumstances have been seen projected on the corona as a background,[†] thus proving the considerable intensity of the coronal light in the immediate neighbourhood of the sun. Unfortunately my attention was almost exclusively directed to the possibility of seeing *bright* lines or I might have frequently repeated the observations of June 10 and so have learnt if the Fraunhofer lines were visible to an equal extent in every direction. It is evident that the upper limit of the light seen by me on the morning of the day mentioned, and which I estimated as about 5′ from the sun's limb, was particularly sensitive to any impurity of the atmosphere, for the slight amount of dust in the air in the afternoon sufficed to fill the whole length of the slit, or upwards of 10′, with strong light.

As a description of what could be seen without a telescope in a favourable interval in the *rainy season* I transfer the following from my note-book :—

> "Vincocaya, 1883, March 14. 'Sky cleared up very quickly. . . . Examined the sun's neighbourhood by bringing his disk behind the edge of the roof or behind a telegraph pole, using coloured glasses or a neutral-tint wedge. About $\frac{3}{4}$° from the sun's limb begins a rapid increase of brightness but I could detect no detail of structure such as is shown in the photographs of the eclipsed sun. Sky, after all, very white in the quarter of the heavens next the sun.' This experiment was tried repeatedly during both visits to Vincocaya and also at Puno."

I also applied the slit of the Vogel Spectroscope to the eye lens of a large opera-glass but without any satisfactory result, although in this way I had been able to see the carbon bands in the spectrum of the Great September Comet of 1883 from the deck of the mail steamer on the voyage to Jamaica. Nor did I neglect to test the diffused light near the obscured sun with the simple spectroscope used on the integrating plan, exactly as if I had been observing an aurora, but always with the same negative results. Still it ought to be mentioned that the appearances produced by the illuminated atmosphere were often of the most tantalizing description, giving again and again the impression that my efforts were about to be crowned with success. On this account did I regret that my limited outfit did not include some simple form of photographic apparatus so as to permit of calling in the aid of this powerful adjunct, which I have since learnt, in common with the rest of the world, has been so efficient for this very purpose in the hands of Dr. Huggins under far less favourable atmospheric circumstances than occur in the Andes. I was perhaps also at a disadvantage in never having been so fortunate as to witness a total solar eclipse.

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[*] Annuaire pour l'an 1884, publié par le bureau des longitudes, p. 858.

[†] In this connexion I would draw the attention of astronomers who may chance to be near the line of simple contact during a solar eclipse to the desirability of looking for the moon's disk outside the sun. Jamaica, for example would be a favourable station on the occasion of the eclipse of March, 16, 1885. The inferior conjunction of Venus in December, 1890, and certain inferior conjunctions of Mercury may also be of interest in this respect.

The observations of June 17th go far to prove that the great groups of lines that make up B, *a* and A are really of solar origin, otherwise they ought to have been greatly enfeebled at Vincocaya. The 19th I devoted more especially to an examination of the upper end of the spectrum carefully focussing on the sun's limb. As has been said, in this way I observed prominences in the "near G" or Hγ line; after which I repeatedly saw them in this tint with the greatest facility.

Respecting the visibility of the sun's spots by simple projection, I find from J. Fabricius' *De Maculis in Sole observatis et apparente earum cum sole conversatione narratio*, Viteb. 1611; that both Johann Fabricius and his father David followed several spots in this way after having found the first by the aid of a small Dutch telescope early in the year 1611.

Meteorological Observations and Results.

During my stay at various places in South America I kept a kind of running meteorological diary, noting down the temperature, &c., as often as convenient without tying myself to regular hours of making the readings. Indeed without a companion or assistant of any kind whatever, an attempt to have adhered to any of the usual sets of hours of observation would have proved very irksome, besides greatly interfering with other objects of my journey. Doubtless certain days might have been set apart as term days and devoted solely to meteorological work, but even then it is questionable whether any moderate number of such days would have afforded more available materials for obtaining a preliminary idea of the climatological conditions of the district visited, than those contained in my irregular journal, which were secured, so to say, without any appreciable sacrifice of time.

The observations were made chiefly at three places: La Paz, first floor of Gran Hotel, February 12–25, 1883; elevation 11990 feet; Vincocaya,[*] Hotel Impresa or Railway Station, February 28–March 16, and June 5–27, 1883; elevation 14360 feet; Puno, first floor of Hotel Inca, March 20–June 2, 1883; elevation 12608 feet.

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[*] I here insert the few approximate latitudes and longitudes found by the box-sextant and pocket chronometer. The initial longitude is that of Mollendo in the Conn. des temps for 1884—

	Lat.	Long. W. of Greenwich.	
Puno	– 15° 50′ 2″	70° 3′·3	Hotel Inca.
Vincocaya	– 15 53 56	71 17·7	Railway Station; booking office.
Arequipa	– 16 25 20	71 32·2	Railway Station; General Superintendent's House.
Mollendo	– 17 2 54	[72 1·8]	Hotel "Quatro Julio."
Tacna	– 18 1 21	. . . .	Court of the N.E. corner house at the intersection of the Calle de Teatro with the Callo de Prado.

The instruments used were—

No. 1. A quicksilver Thermometer, A 2647, Negretti and Zambra.

No. 2. A pair of wet and dry bulb Thermometers, 926 $\frac{\text{A.M.D.}}{9}$, by Negretti and Zambra.

No. 3. A black-bulb Thermometer in approximate *vacuo* $\frac{\text{A.M.D.}}{94}$, Negretti and Zambra.

No. 4. A Pocket Aneroid Barometer, Casella, 4218.

No. 5. A boiling-point Thermometer, Casella, 18072. This instrument was divided to tenths of a degree Fah.

No. 1 was supplied to the Transit of Venus expedition to Jamaica. Its corrections were found at Dun Echt from comparisons with a well-tested instrument. The corrections of Nos. 2 and 3 were taken from small tables determined at Kew. These instruments were obligingly lent by the Ordnance Department at Jamaica.

For number 5 there exist as yet only corrections found at Dun Echt from examinations of the boiling-point with the standard barometer of the observatory: these give a correction of — 0°·11 near 212° which has been adopted as constant for the whole scale.

No. 4 was compared repeatedly with 5 on the journey, with the following results:

		Boiling Point.	Aner. Bar. in.	Correction. in.
1883, Mar. 6,	Vincocaya,	186°·29	17·55	— 0·02
„ 8,	„	186·05	17·47	— 0·03
„ 15,	„	186·25	17·45	+ 0·03
Apr. 12,	Puno,	189·68	18·65	+ 0·17
„ 16,	„	189·60	18·62	+ 0·17
May 11,	„	189·55	18·63	+ 0·14
June 14,	Vincocaya,	186·22	17·32	+ 0·14
„ 25,	„	186·15	17·26	+ 0·18
July 1,	Mollendo,	211·96	29·84	— 0·01
„ 2,	„	211·85	29·82	— 0·05
„ 8,	Tacna,	208·45	27·87	— 0·06

It is nearly perfectly compensated; warming to the extent of 17° causing it to rise 0·01 inch.

As corrections to the barometer, I have adopted for La Paz + 0·16in., at Puno + 0·15in., at Vincocaya 0·00 from Feb. 28 to Mar. 17 and + 0·16in. from June 6 to 27; and at Arequipa + 0·08in. for June 29 and 30.

All the readings of the thermometers have been corrected in accordance with their respective tables: unfortunately that for number 3 did not extend above 110°, nor did it occur to me that I might have calibrated the upper part of the tube, until after the instrument had passed out of my hands. But as this precaution was taken with the lower part of the scale of No. 1, and as Nos. 1 and 2 were frequently compared with each other in cases of doubt, the low temperatures noted at Vincocaya rest on a sure basis.

Respecting the exposure of the thermometers it may be well to say a few words: sometimes they were merely hung outside a window while at others they were suspended from the supporting pillar of the roof of a balcony, or occasionally swung about in the outside air until a satisfactory reading had been obtained; but under no circumstances was a record made if the exposure was considered unsatisfactory. The wet-bulb thermometer in this respect frequently required very judicious treatment, particularly whenever the temperature of evaporation fell far below the freezing point. I then usually conveyed successive drops of water to the covered bulb by means of a slender bit of wood or a lead pencil until the glazing of the muslin showed that it was covered with ice. The reading was then made a few minutes later, when the mercury had ceased to fall. The black-bulb thermometer was usually placed on, or close to the ground. The aneroid barometer was carried in the pocket in the day-time but always in its outer leather-covered case. It was invariably held horizontally and gently tapped before reading off, which was done with the aid of a lens of about two inches focus.

To utilize the temperatures recorded at Puno so as to form a connecting link between those at La Paz and Vincocaya in so far as the height above the sea is concerned, and between the two sets at the latter place with regard to change of season, I have divided the observations at Puno into three groups of which the second extends from April 15 to May 8. To combine the readings of the dry thermometer in each group so as to show the daily period and mean temperature they were first corrected for instrumental errors and referred, in common with all the meteorological observations, to *apparent* time. This latter precaution being quite necessary on account of the strongly marked daily periods that manifested themselves. The readings were then arranged in sets of four in sequence of time of the day and the means taken. These means combined graphically then gave a curve representing the daily change of temperature which was read off for each hour. The average of the hourly quantities thus obtained has been adopted as the mean temperature. The drawing of these curves for parts of the twenty-four hours in which there were but few and scattered records was much facilitated by beginning with Puno as the station for which the most complete materials were available. Further, by noticing that the temperature fell with great uniformity throughout the night and that the minimum, as might have been expected, occurred shortly before sunrise it was easy by analogy to lay down fairly satisfactory curves for those places at which very few temperatures were recorded. The reliability of this method was confirmed by finding, amongst my letters or other papers, stray records of temperature, which, not having been entered in the meteorological journal could be used to test the curves. In no case did the introduction of these additional numbers necessitate a material alteration in any of the curves.

In the following tables each number is the mean of four observations unless there is a note to the contrary :—

La Paz.

Mean shade temperatures, 1883, February 12–25.

App. time. H. M.	Fah.	App. time. H. M.	Fah.	App. time. H. M.	Fah.	App. time. H. M.	Fah.
0 17 a.m.	45°·5*	9 50 a.m.	56°·8	12 46 p.m.	60°·1	6 18 p.m.	52°·5
6 46	43·6†	10 12	56·4	1 42	63·3	7 39	51·4
7 21	47·9	10 32	54·8	2 15	68·0‡	8 40	48·8
7 46	49·6	10 55	59·1	3 25	60·0	8 46	49·1
7 57	49·8	11 12	56·9	3 46	59·2	9 42	48·5
8 28	52·6	11 19	59·8	4 30	58·5	10 36	48·0
9 5	53·7	11 54	59·0	5 12	56·5	11 16	46·8‡

* One reading only. † Mean of 2 readings. ‡ Mean of 3 readings.

Vincocaya.

Mean shade temperatures, Period I., 1883, February 28–March 16.

App. time. H. M.	Fah.	App. time. H. M.	Fah.	App. time. H. M.	Fah.	App. time. H. M.	Fah.
3 17 a.m.	29°·6	10 39 a.m.	46°·6	3 59 p.m.	40°·6	8 3 p.m.	34°·2
4 37	29·7	11 12	50·9	4 21	35·4	8 43	30·8
5 44	28·9	11 44	51·6	4 43	38·9	9 10	35·0
6 36	33·0	12 32 p.m.	47·0	5 6	36·5	9 52	31·9
7 25	34·6	1 36	47·1	5 52	34·8	10 39	35·1
8 21	39·7	2 8	48·2	6 13	34·8	11 8	31·5
9 21	47·8	3 9	40·5	6 48	34·1		
10 2	53·2	3 32	44·0	7 35	33·7		

Puno.

Mean shade temperatures, Period I., 1883, March 20–April 14.

App. time. H. M.	Fah.	App. time. H. M.	Fah.	App. time. H. M.	Fah.	App. time. H. M.	Fah.
0 25 a.m.	41°·6	11 13 a.m.	59°·9	2 59 p.m.	59°·4	6 36 p.m.	47°·2
1 28	40·2	11 22	59·3	3 25	56·2	7 18	47·2
3 38	39·2*	11 38	63·5	3 41	53·6	7 54	44·8
5 32	35·4†	11 54	61·4	4 2	56·0	8 32	44·2
6 33	41·0‡	12 10 p.m.	62·0	4 17	55·8	9 8	46·5
7 19	44·0‡	12 21	59·8	4 38	55·9	9 28	44·3
8 40	57·0	12 35	62·3	5 7	50·1	9 52	43·2
9 10	58·9	12 57	61·9	5 21	50·0	10 21	44·7
9 44	56·3	1 14	63·3	5 25	49·8	10 42	42·8
10 10	57·5	1 37	62·3	5 32	51·7	10 55	43·2
10 38	63·1	2 4	59·7	5 46	49·7	11 32	43·0
10 41	59·9	2 26	58·2	6 3	48·0	11 46	41·7*
11 3	60·0	2 42	58·9	6 16	48·8		

Puno.

Mean shade temperatures, Period II., 1883, April 15–May 8.

App. time. H. M.	Fah.	App. time. H. M.	Fah.	App. time. H. M.	Fah.	App. time. H. M.	Fah.
0 16 a.m.	37°·4	11 17 a.m.	59°·4	2 25 p.m.	58°·3	7 11 p.m.	46°·8
0 31	39·4	11 26	58·7	2 48	57·9	7 47	42·5
0 46	36·2	11 34	58·7	3 18	53·6	8 41	44·3
2 0	40·3	11 48	59·9	3 55	57·4	9 33	44·0
5 32	32·5‡	12 4 p.m.	59·4	4 17	53·7	10 6	42·6
7 44	41·7‡	12 18	59·0	4 26	52·7	10 38	40·6
8 42	50·0	12 32	59·7	4 54	53·1	10 58	39·1
9 35	54·5	1 3	60·2	5 13	52·7	11 18	42·1
9 46	54·0	1 16	59·2	5 32	50·8	11 37	40·0
9 59	57·3	1 29	60·6	5 44	49·8	11 56	38·8
10 13	59·0	1 40	59·8	5 54	51·2		
10 32	57·5	1 54	61·0	6 16	47·4		
11 1	57·3	2 8	60·4	6 38	46·8		

* Mean of 3 readings. † Mean of 2 readings. ‡ One reading only. All the rest are means of 4 readings.

Puno.

Mean shade temperatures, Period III., 1883, May 9–June 2.

App. time.	Fah.	App. time.	Fah.	App. time.	Fah.	App. time.	Fah.
H. M.		H. M.		H. M.		H. M.	
1 29 a.m.	35°·8*	11 55 a.m.	57°·1	4 17 p.m.	53°·3	7 17 p.m.	44°·7
7 39	43·9	12 15 p.m.	57·3	4 32	52·7	7 48	44·6
8 43	51·3	12 37	58·7	4 49	51·4	8 29	45·2
9 30	52·9	1 7	59·7	5 1	49·1	9 17	42·0
9 52	53·2	1 17	56·9	5 15	49·6	9 40	42·7
10 4	54·6	1 26	56·5	5 26	47·4	9 52	41·8
10 20	54·5	2 2	57·1	5 34	50·1	10 8	42·8
10 44	56·0	2 22	57·6	5 46	50·3	10 27	38·9
11 13	58·7	2 40	53·3	6 0	49·0	10 41	41·8
11 26	55·5	3 34	55·7	6 9	46·3	11 7	40·7
11 32	56·9	3 44	52·4	6 34	48·6	11 16	41·4
11 37	57·7	4 0	54·8	6 58	44·7	11 32	40·8†

Vincocaya.

Mean shade temperatures, Period II., 1883, June 6–27.

App. time.	Fah.	App. time.	Fah.	App. time.	Fah.	App. time.	Fah.
H. M.		H. M.		H. M.		H. M.	
0 10 a.m.	16°·2	8 50 a.m.	28°·5	2 15 p.m.	44°·4	9 12 p.m.	24°·2
5 8	11·3†	9 17	31·2	3 59	43·3	9 58	20·5
6 32	11·8	9 49	35·0	5 33	37·8	10 10	21·6
6 58	17·9	10 44	40·8	7 6	29·3	10 59	18·0‡
7 43	21·9	11 58	45·8	7 30	27·0	11 37	18·8‡
8 17	22·9	12 38 p.m.	47·2	8 18	24·6		

Tacna.

Mean shade temperatures, 1883, July 7–10, with corresponding values for Santiago—§

H.	M.	Tacna.	Santiago.	T. — S.
10	26 a.m.	57°·9†	50°·1	+ 7°·8
11	50	62·0†	52·9	+ 9·1
12	58 p.m.	62·8†	55·1	+ 7·7
3	29	62·4†	54·0	+ 8·4
4	3	60·3†	53·1	+ 7·2
6	28	55·4*	48·9	+ 6·5
10	17	54·3†	45·5	+ 8·8

I here insert the scanty records made at Arequipa on two occasions, and at Mollendo, adding, where necessary, the resultant tension of aqueous vapour and the relative humidity, as well as a few stray memoranda.

* Mean of 2 readings. † Mean of 3 readings. ‡ Mean of 5 readings.

§ To utilize the few thermometer readings made at Tacna, I have compared them with the results obtained at about the same height above the sea at Santiago de Chili. From the tables on page 380 of *The U.S. Naval Expedition to the Southern Hemisphere*, Vol. VI., I laid down a curve showing the temperature at Santiago for July 7·5, from which were taken the numbers given in the Santiago column of the text. The mean of the differences is + 7°·9. The Santiago curve raised by this quantity has been adopted for Tacna on p. 219.

OBSERVATIONS at AREQUIPA, HOTEL TEATRO (A. I.), Feb. 2 to 8, 1883.

App. time. H. M.	Bar. in.	Ther.	App. time. H. M.	Bar. in.	Ther.
3 31 a.m.	—	55°·9	8 46 a.m.	—	61°·9
5 46 „	—	52·8	9 16 „	—	64·8
6 16 „	—	52·2	5 16 p.m.	—	62·4
6 26 „	—	51·8	10 6 „	22·66	58·5
6 46 „	22·60*	55·9	10 16 „	22·64	55·9
7 16 „	—	56·1	11 46 „	22·58	55·1
8 31 „	22·61	59·4			

OBSERVATIONS at AREQUIPA RAILWAY STATION (A. II.), June 29, 30, 1883.

App. time. H. M.	Bar. in.	Dry Ther.	Wet Ther.	Tension of Aqueous Vapour. in.	Relative Humidity.
6 38 a.m.	22·30	32°·1	26°·5	·096	53%
8 56 „	—	49·9	45·0	·251	70
11 44 „	22·34	67·7	48·1	·142	21
12 8 p.m.	22·36	68·7	48·6	·143	20
1 16 „	—	69·7	49·5	·154	21
10 52 „	22·35	35·2	—	—	—
11 48 „	—	33·9	28·5	·110	56

At 1^{h} 11^{m} p.m. the black-bulb thermometer showed 205°·4, the mercury filling the tube. The weather was superbly fine.

OBSERVATIONS at MOLLENDO, HOTEL QUATRO JULIO, July 2, 1883.

App. time. H. M.	Bar. in.	Dry Ther.	Wet Ther.	Tension in.	Relative Humidity.	Cloud in tenths of whole sky.
9 53 a.m.	29·87	65°·7	59°·8	·436	69%	cum 8
10 23 „	29·88	68·7	61·1	·439	63	cum 5
11 32 „	29·86	68·3	60·6	·426	62	—
12 10 p.m.	29·89	68·4	61·0	·437	63	—
1 29 „	29·80	68·8	61·3	·442	63	—
1 59 „	29·79	66·8	60·5	·443	67	cir-c 1
4 29 „	29·79	62·5	58·3	·432	76	—
5 13 „	29·81	61·8	58·1	·435	79	cum 5
7 53 „	29·87	60·3	57·0	·422	81	overc 10
11 29 „	29·86	59·6	56·1	·405	79	overc 10

In accordance with what has already been said, the foregoing data were used in the construction of curves representing the diurnal oscillations of temperature, from which were read off the numbers in the following table, where uncertain results are enclosed in brackets, while still more hypothetical quantities are marked with a note of interrogation.

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* The readings of the barometer on this visit to Arequipa are uncorrected. Three readings at the Railway Station—8^{h} 46^{m} a.m., 22·71 in.; 2^{h} 16^{m} p.m., 2·72 in.; 4^{h} 31^{m} p.m., 22·68 in.—give an approximate height of the first floor of the hotel above the station platform of 148 feet, a result possibly somewhat in excess of the truth.

TABLE SHOWING THE SHADE TEMPERATURE FOR EACH HOUR OF THE DAY AT SEVERAL STATIONS ON OR NEAR THE ANDES.

—	Stations.									
	La Paz.	Vincocaya.	Puno.	Puno.	Puno.	Vincocaya.	Arequipa, Hotel Teatro.	Arequipa Railway Station.	Mollendo.	Tacna.*
Period, . .	—	I.	I.	II.	III.	II.	I.	II.	—	—
Mean Epoch, .	Feb. 19·5	March 10·5	April 4·2	April 26·5	May 19·3	June 15·5	Feb. 6	June 29·5	July 2	July 8·5
Obs., . .	105	120	194	190	189	93	13	7	10	20
Height in feet,	11990	14360	12608	12608	12608	14360	7698	7550	66±	1837
Hour.										
1 a.m., .	45°·2	31°·4	40°·8	38°·5	38°·1	16°·1	[55°·5]	33°·6 ?	59°·1 ?	51°·7
2 ,, .	44·3	30·9	39·7	37·2	37·0	15·0	[54·8]	33·3 ?	58·8 ?	51·3
3 ,, .	43·3	30·3	38·6	36·0	35·8	13·8	[54·2]	33·0 ?	58·6 ?	50·8
4 ,, .	42·4	29·7	37·5	34·8	34·7	12·6	[53·6]	32·8 ?	58·4 ?	50·3
5 ,, .	[41·5]	29·1	36·4	33·5	33·5	11·4	53·0	32·6 ?	58·1 ?	49·9
6 ,, .	42·4	29·9	37·3	32·6	33·5	10·9	52·3	[32·3]	57·8 ?	49·3
7 ,, .	45·8	33·8	43·3	38·6	39·8	16·3	55·3	[33·6]	59·0 ?	49·9
8 ,, .	49·8	38·9	49·7	44·9	46·2	22·4	59·3	[42·3]	61·6 ?	52·3
9 ,, .	53·1	44·6	56·0	51·3	51·5	29·4	63·4	50·4	64·0 ?	54·9
10 ,, .	55·8	49·2	59·6	56·5	54·1	36·2	67·5 ?	[57·2]	66·7	57·0
11 ,, .	58·3	50·7	60·9	58·8	56·4	42·4	71·2 ?	[64·3]	68·7	59·2
Noon, . .	60·6	50·2	61·4	59·5	57·8	46·5	72·8 ?	68·4	69·2	61·1
1 p.m., .	62·9	48·6	61·4	59·9	58·1	46·8	73·2 ?	69·6	68·9	63·1
2 ,, .	64·3	46·1	60·7	59·9	57·5	45·7	72·0 ?	[68·9]	67·6	63·7
3 ,, .	62·6	43·1	57·9	57·5	55·5	44·4	70·6 ?	65·0 ?	[65·7]	62·6
4 ,, .	59·5	39·4	55·0	54·8	53·3	42·7	67·2 ?	58·8 ?	63·6	61·1
5 ,, .	56·8	36·0	52·1	52·0	50·5	39·5	63·4	52·0 ?	61·8	59·4
6 ,, .	54·3	34·7	49·2	49·2	47·8	34·7	[60·2]	45·8 ?	[60·9]	57·6
7 ,, .	52·1	34·2	46·9	46·5	45·6	29·6	[58·8]	41·5 ?	[60·6]	56·2
8 ,, .	50·1	33·8	45·8	44·6	43·8	26·2	[58·4]	39·4 ?	60·3	55·1
9 ,, .	48·8	33·3	44·8	43·3	42·6	23·6	[57·9]	37·8 ?	[60·1]	54·3
10 ,, .	47·9	32·8	43·8	42·1	41·5	21·2	57·5	[36·3]	[59·9]	53·6
11 ,, .	47·0	32·4	42·8	40·9	40·4	19·0	57·2	34·8	59·7	52·9
Midnight, .	46·1	32·0	41·8	39·7	39·2	17·3	56·8	33·8	59·4	52·2
General mean,	51·4	37·3	48·5	46·4	45·6	27·7	61·1 ?	[45·7]	62·0	55·4
Day ,,	57·0	42·9	55·6	53·6	52·4	37·2	66·3 ?	[56·4]	64·8	58·5
Night ,,	45·9	31·7	41·4	39·1	38·8	18·1	[55·8]	[35·1]	59·2	52·3
Average minimum,	[41·1]	28·9	35·8	32·4	32·8	10·6	52·2	32·1	—	49·3
at	$5^h 28^m$	$5^h 27^m$	$5^h 38^m$	$5^h 53^m$	$5^h 45^m$	$5^h 48^m$	$6^h 10^m$	$6^h 38^m$	—	$6^h 12^m$
Average maximum,	64·3	50·8	61·5	60·1	58·1	47·1	73 ?	69·7	68·3	63·8
at	$2^h 6^m$	$11^h 12^m$	$12^h 33^m$	$1^h 38^m$	$12^h 54^m$	$12^h 33^m$	$12^h 45^m$	$1^h 15^m$	$12^h 30^m$	$1^h 42^m$
Absolute min.,	42·0	25·2	34·1	32·5	34·8	6·9	51·8	32·1	—	—
Absolute max.,	69·6	57·6	65·9	65·7	64·2	50·0	—	69·7	68·8	64·7

* See note on page 217. The height is from Conn. d. t. (Pentland)

This table may be adopted as fairly representing the diurnal change of temperature at the seasons and places indicated. In addition to the general mean and the average maximum and minimum, I have given the mean day temperature from 7 a.m. to 6 p.m. inclusive, and that for the night from 7 p.m. to 6 a.m., to which I shall refer further on. I have also added the extreme temperatures recorded at each station.

One of the most interesting deductions that can be made from this table is the rate of decrease of temperature in ascending through the atmosphere. The quantities however are not in a very available form for this purpose, since to make the best of them the results for the southern stations require to be somewhat increased to refer them to the latitude of Puno and Vincocaya. Now the difference of mean temperature between Tacna and Santiago in the early part of the month of July is at the rate of 0°·513 Fah. for each degree of approach to the equator, therefore the results for Mollendo and Tacna may be fairly raised by + 0°·6 and + 1°·1, while in the same way 0°·3 may be added to the second group at Arequipa. The observations at La Paz and the first group at Arequipa having been made when the sun was very nearly vertical may be safely left without any correction for latitude. To allow for the advance of the seasons the third series at Puno may be lowered by one degree before comparison with the second series at Vincocaya. On the same grounds the temperatures at Vincocaya in June, which were still appreciably sinking, have been lowered by 1°·6 (or ⅙th of the change between the two visits to that station) before comparison with A. II. and M. I have assumed the same change at A. II. when comparing it with the corrected means of the last group at Puno. It is only necessary to add that the difference of temperature between La Paz and Arequipa I. was taken at 9°·54 from a mean of eight times in the day for which the single readings gave a sufficient guarantee, La Paz being the cooler station and the conditions of weather and exposure closely alike at both stations. The following table and the nomenclature of the stations will now be readily understood :—

TABLE SHOWING THE CHANGE IN HEIGHT, IN ENGLISH FEET, CORRESPONDING TO A DECREASE OF TEMPERATURE OF 1° FAH.

	1	2	3	4	5	6	7	8
Upper Station, .	V. I.	V. II.	La P.	V. II. − 1°·6	A. II. + 0°·3	T. + 1°·1	P. III. − 1°·0	V. II. − 1°·6
Lower Station, .	$\frac{\text{LaP.} + \text{P.I.}}{2}$	P. III. − 1°	A. I.	A. II. + 0°·3	T. + 1°·1	M. + 0°·6	A. II. + 0°·3 + 1°·6	M. + 0°·6
	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
Difference of height,	2061	1752	4292	6810	5713	1771	5058	14294
Mean height, . .	13330	13484	9844	10955	4694	952	10079	7213
Feet per 1° Fah.— From Mean temp.,	162	104	443	—	—	290	—	392
,, Day ,,	154	123	—	—	—	305	—	480
,, Night ,,	173	89	—	—	—	277	—	330
,, average Min.,	215	83	—	291	317	—	2299	—
,, ,, Max.,	170	175	—	278	− 112	443	349	611
,, Mean of Max. and Min.	—	—	—	284	886	—	606	—

Of the eight columns of this table the seventh is really a duplicate made up of 2 and 4. To emphasize the mean results they have been underlined, whether they have been derived from the mean temperatures or, failing these, only from the means of maximum and minimum. Commencing with the lower stations it may be remarked that the 290 feet per degree in column 6 is fairly normal, as both stations were below the level of the chief cloudy stratum. Column 5 shows most strikingly the effect of passing through this stratum, for the mean value is no less than 886 feet for 1°, while during the hotter part of the day, from 11 a.m. to 3 p.m. the upper station is actually the warmer, as may be seen by the table on p. 219, and also in the negative result derived from the mean of the daily *maxima*. Column 7 is introduced to show that these conditions are felt above Arequipa, but to a less degree. The gradual progress of the change is further illustrated by column 3, while the first and second columns show the remarkable rate at which the cold increases when a considerable height is attained. This is fully borne out by the result in the fourth column. The thermal conditions at the Railway Station at Arequipa (A. II.) are so remarkable as to deserve a few words of comment. Situated on the floor of a great valley sheltered to the N., N.E. and E. by the great mountains of Charchani, Misti and Pichupichu, whose summits, according to the railway survey, attain to the respective heights of 19800, 18650 and 17800 feet, it is not only protected from the cold winds that blow over the elevated plains of the interior but in the daytime the direct heating effect of the sun is reinforced by the rays reflected or radiated from the surrounding slopes. On the other hand, at night it is exposed to the flow of cold air from the impending slopes of the Misti, whose gigantic cone rises upwards of 8° above the horizon although its highest point is about fifteen miles distant. To these causes are due the great range of 37°·6 in the temperature on June 29 and 30.

The law of thermal decrease in our atmosphere being of so great importance in the study of astronomical refractions and in the determination of heights from the pressure of the air, I here insert for comparison a small table from Schmid's *Lehrbuch der Meteorologie*, p. 428, derived from the materials collected by Humboldt in his paper on isothermal lines in the *Kleinere Schriften*, Vol. I., pp. 296 *et seq.* The numbers have been transposed into British feet and degrees Fahrenheit.

Decrease of Temperature of 1° Fah. consequent on an increase of height—

	Feet.		Feet.		Feet.
between	0	and	3281	of	310
	3281	,,	6562	,,	536
	6562	,,	9843	,,	423
	9843	,,	13124	,,	239
	13124	,,	16404	,,	328

therefore on the mean 1° in 341 feet.

Here we again have the phenomenon of a striking abatement of the rate of decrease between 3281 and 6562 feet, followed by an accelerated increase until a height of 13124 feet (or 4000^{m}) is attained. The subsequent second falling off in the decrement between 13124 and 16404 feet would be very remarkable if it were sufficiently well guaranteed. But on consulting pages 297 and 298 of the *Kleinere Schriften* it will be seen that whereas Humboldt and Caldas had collected satisfactory materials in the shape of mean temperatures at 30 stations from the sea-level up to 3079^{m} or 10102 feet, they only give two means above that height—the *Paramos* or great elevated plains at 11483 feet (3500^{m}) with a mean temperature of 47°·3, and that of 34°·9 assigned to the lower limit of perpetual snow at a height of 4800^{m} or 15748 feet. It need hardly be said that both of these averages are somewhat vague from the very nature of the cases, the heights as well as the temperatures being uncertain within considerable limits. Nor are they supported by the five typical examples given on page 296 of the work quoted. These are reproduced in the following table, where is also shown the course I have taken to obtain the mean temperature for the whole twenty-four hours at each station. For Micuipampa the mean has naturally been adopted, while for Nevado de Toluca I find the record to be 7°·1 higher than the average of the columns for La Paz and the three periods at Puno on p. 219. The average of the mean temperatures in those four groups is 48°·0, which lessened by 7°·1 gives 40°·9 as the concluded mean temperature on the Nevado de Toluca in the month of September. The other stations have been treated in a similar manner. The data for effecting a correction for the season of the year are wanting.

TABLE SHOWING THE REDUCTION OF HUMBOLDT'S TYPICAL EXAMPLES ABOVE 3500 METRES.

Humboldt and Caldas.	Reduction.	Concluded mean tem. Fah.	Height (Humboldt) Feet
Micuipampa, day 5° to 9° C., night + 2° to — 0°·4 C.	The mean adopted.	39°·0	1187.
Nevado de Toluca, Sept., noon, 11°·5 C.	$\frac{1}{4}$(La P. + P. I., II. and III.) — 7°·1	40·9	12182
Coffer de Perote, Feb. 9 a.m., 10°·2 C.	$\frac{1}{4}$(La P. + P. I., II. and III.) — 2°·6	45·4	12139
Pic del Frayle, Sept., noon, 4°·3 C.	$\frac{1}{2}$(V. I. + V. II.) — 8°·6	23·9	15161
Chimborazo, June, 1 p.m., — 1°·6 C.	$\frac{1}{2}$(V. I. + V. II.) — 18°·6	13·9	19292

On combining the three first of these stations into one mean and the last pair into another, we have—

	Mean temperature.	Height in feet.
Lower group,	41°·8	12064
Upper ,,	18·9	17226

or a decrement of 1° F. for every 225 feet at a mean elevation of 14600 feet; that is to say nearly the same value as Humboldt found for the thousand metres below 13000 feet, but fully 100 feet less than the height deduced from the adopted temperatures on the *Paramos* and at the snow-limit. It seemed therefore desirable to see whether it is supported by recent observations; these fortunately were at hand.

In the year 1880, Mr. Edward Whymper ascended most of the loftiest peaks in Ecuador and took readings of the thermometer on their summits.* As a rule the hour of the day is not given. This is very unfortunate and probably results from the idea entertained by Mr. Whymper that the temperatures encountered by him were altogether abnormal, or as he puts it "quite improper, and contrary to our preconceived notions as to what ought to occur on the top of a snow mountain 19000 feet high." The exceptions are for the first ascent of Chimborazo and for the arrival on the summit of Cayembe, which for special reasons was scaled very early in the day. Assuming 2 p.m. as a likely hour for reaching the summit of a high mountain and deriving the mean daily temperature from Mr. Whymper's observations by the aid of the averages of both visits to Vincocaya, I find—

Reduction of Mr. Whymper's Observations.	Mean temp. Fah.	Height (Whymper).
Chimborazo, 5 p.m., 21° Fah., ½(V. I. — 15°·0) + ½(V. II. — 18°·5),	16°·8	20517
Corazon, 37° to 43°, ½(V. I. + V. II.) — 5°·9,	26·6	15871
Cotopaxi, 13° to 21° during 26 hours, mean adopted,	17·0	19550
Antisana, 44° to 60°, ½(V. I. + V. II.) + 6°·1,	38·6	19260
Pichincha, 46°, ditto — 0°·1,	32·6	15918
Cayambe, 10 a.m. and (say) 12 noon, 32° to 41°, ditto — 9°·0,	23·5	19200
Sara-Urcu, 43° to 55°, ditto + 3°·1,	35·6	15500
Cotocachi, 36°, ditto — 9°·9,	22·6	16289
Carihuairazo, 38° to 42°, ditto — 5°·9,	26·6	16480
Chimborazo, 2nd ascent, 15° to 20°, ditto — 28°·4,	4·1	20517

In these numbers there is an obvious, although quite fortuitous compensation between the two extreme results for Antisana and the second ascent of Chimborazo, but it is one that might also very easily be brought about in great part by the use of the actual hours of observation. The heights naturally divide themselves into two clearly marked groups; taking the means and adding Humboldt's mean temperature for Quito combined with Mr. Whymper's height for that city, we have—

	Temperature.	Height in feet.
1st group,	20·0	19809
2nd group,	28·8	16012
Quito,	57·9	9350

The difference between the two elevated groups gives a decrease of 1° F. in 431 feet, while the second group and Quito are reconciled by 1° for every 229 feet, and finally the mean of the mountain stations combined with Quito give a decrement of 1° for 256 feet at a mean elevation of 13630 feet. This result differs somewhat from that obtained from Humboldt's typical examples and possibly might not be very materially altered by a revision of Mr. Whymper's temperatures by the aid of the hours of observation, however much such a recomputation might affect the difference between the two upper groups and modify the anomalous ratio indicated by them. Considering the uncertainty of that ratio it seems not unlikely that a mean decrement of 1° Fah. for every 240 feet closely represents the mean distribution of temperature on the slopes of high mountains. Nor would such a ratio be greatly at variance with the conclusions arrived at on p. 220 if a hypothetical station about 10000 feet high were introduced into the table there given. Nevertheless the results for the higher stations in that table point irresistibly to a more rapid rate of decrease of temperature wherever a cloudless winter makes itself felt.

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* Nature, XXIII., p. 323. Proc. Roy. Geog. Soc., Aug. 1881, p. 461.

As a hint to travellers I would point out that isolated observations about 8 30 a.m. or 6 30 p.m. *with an exact record of the time* would give a close approximation to the mean temperature of the locality. Such an approximation might also be obtained by self-registering maximum and minimum thermometers if properly exposed. By ascending the same mountain twice or oftener and establishing registering thermometers on the first ascent, invaluable records might be obtained in a very short time. It may also be remarked here that the observations at Puno and La Paz indicate a strong tendency towards the establishment of a mean temperature every twenty-four hours, so that if the day is fine and warm with a clear transparent atmosphere the night is correspondingly cold. In thick weather a sort of compensation occurs; the days are cool and the nights relatively warm. A further peculiarity is that moments can be frequently selected when the temperature is exactly the same at stations differing in elevation by several thousands of feet, or in extreme cases the upper station may have the higher shade temperature. Arequipa compared with Tacna or even Mollendo in the heat of the day affords an instance of this, still further exaggerated by the influence of the lower cloud stratum. Isolated observations at such times would obviously be of no use in the solution of the problem of the mean decrease of temperature with increasing elevation.

Psychrometrical Results.

Returning from this long digression, the hygrometrical results call for attention. From the depressions of the wet-bulb thermometer were computed the tension of aqueous vapour and also the percentage of relative humidity by Guyot's tables in the Smithsonian Collection, with a slight extension where necessary.* The tension and humidity varied so much that where sufficient observations were available they have been combined into means of 16 readings. The wet-bulb thermometer was not taken to La Paz. In the subjoined tables are exhibited the mean results for Puno and Vincocaya. A few determinations for other stations have already been given.

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* It may be well to remark that the constant in the denominators of three of the psychrometrical formulæ in page B 47 of that collection ought obviously to be 1272·2 in place of 1240·2. The corresponding tables in the collection have been computed with the right constant.

Tables of the Tension of Aqueous Vapour and of the Relative Humidity of the Air.

Vincocaya.
1883, March 7–16. Rainy season.
Means of 4 observations.

App. time.		Tension.	Rel. Hum.
H.	M.	in.	%
7	18 a.m.	0·171	89
9	25	0·24	70
10	51	·216	66
11	23	·225	63
12	18 p.m.	·224	60
1	48	·215	68
3	13	·214	80
4	21	·194	90
5	29	·184	90
6	26	·183	99 *

Vincocaya.
1883, June 16–26. Dry season.
Means of 4 observations.

App. time.		Tension.	Rel. Hum.
H.	M.	in.	%
8	10 a.m.	0·052	45
9	38	·097	50
11	46	·146	45
12	47 p.m.	·116	35
2	48	·091	30
4	41	·101	38
9	16	·065	58
10	57	·060	62 †

Puno.
1883, April 10–14.
Means of 4 observations.

App. time.		Tension.	Rel. Hum.
H.	M.	in.	%
9	53 a.m.	0·198	39
11	7	·215	39
12	21 p.m.	·218	39
2	17	·178	35
3	46	·155	35
5	1	·168	43
5	58	·180	52
7	7	·189	58
10	7	·173	64
11	45	·170	65

Puno.
1883, April 15–May 8.
Means of 16 observations.

App. time.		Tension.	Rel. Hum.
H.	M.	in.	%
0	52 a.m.	0·124	54
9	22	·169	42
10	34	·200	40
11	34	·191	38
12	24 p.m.	·184	35
1	30	·197	37½
2	35	·192	40½
4	26	·166	39½
5	40	·189	51
7	18	·163	53
10	19	·154	58
11	35	·151	58 ‡

Puno.
1883, May 9–June 2.
Means of 16 observations.

App. time.		Tension.	Rel. Hum.
H.	M.	in.	%
8	56 a.m.	0·182	50
10	33	·187	41½
11	34	·190	41½
12	44 p.m.	·196	40
2	2	·183	41
3	30	·213	49
4	36	·180	46
5	28	·182	52½
6	21	·189	56½
8	4	·151	51
9	56	·141	54
11	7	·137	54

The considerable decrease of the relative humidity towards noon on the first visit to Vincocaya is merely a consequence of the great rise of temperature outstripping the rate at which the air could be supplied with moisture from the wet ground, for it is accompanied by a large increase in the tension. Something similar is shown during the second stay at that station, but in this case the very low tension in the evening and morning is due quite as much to the coldness of the night as to anything else. This is naturally associated with a rise in the relative humidity immediately after sunset.

The psychrometrical observations at Puno belong altogether to the dry period of the year, and it is remarkable that although the observing station was within about a mile of the great Lake of Titicaca the relative humidity was nearly as low as at Vincocaya in the midst of the Pampas, which at that season are reduced to a series of arid plains intersected at wide intervals by shrunken rivulets.

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* Mean of 3 observations.

† Mean of 5 observations.

‡ Mean of 11 observations.

Before leaving this subject it may not be uninteresting to give the original meteorological notes for the dryest day spent at Puno and also a representative day for each visit to Vincocaya. All the readings are freed from index errors.

Puno, 1883, May 12. Height 12608 feet.

	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.
App. time,	8 56 a.m.	10 32	11 26	12 26 p.m.	1 32	4 44	6 32	10 32
An. Barom.,	18·82 in.	18·82	18·81	18·79	18·75	18·76	18·78	18·82
Clouds,	cir-cu 5	c-c 1	cu ½B *	cu ½B	c-c ½B	cu 1	clear	clear
App. of sky,	*b*2 †	*b*4	*b*4	*b*4	*b*4	*b*2	*b*3	*b*3
Dry Ther.,	51·5	52·5	54·7	56·9	57·1	54·2	44·3	34·8
Wet ,,	39·7	37·5	39·3	39·2	38·3	37·4	31·6	29·0
Concl. Ten.,	0·147 in.	·101	·114	·092	·075	·085	·083	·118
Rel. Hum.	39 %	21	27	20	16	20	28	58

(Black-bulb Ther., April 3, 2^h 12^m p.m., 200°·4; May 15, 9^h 31^m a.m., 195°·1.)

Vincocaya, 1883, March 13. Height 14360 feet.

		H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.
App. time,	3 22 a.m.	7 52	9 52	10 52	11 52	12 52 p.m.	3 22	4 22	6 7	7 52	8 52	9 52	10 52
An. Barom.,	—	17·47	17·48	17·49	17·47	17·45	17·45	17·42	17·45	17·49	17·49	17·50	17·50
Clouds,	—	cu 10	cu 9	cu 9	cu 7	nim 10	nim 10	cu 9	nim 10	nim 10	nim 10	nim 10	nim 10
App. of sky,	—	—	—	*b*, showery	*b*	lightning, thunder, hail.	snow	hazy	snow	snow	snow	snow	snow
Dry Ther.	33·4	37·2	53·2	53·0	57·6	—	—	35·3	30·1	28·7	27·1	27·7	27·4
Wet ,,	—	35·1	—	45·6	48·7	—	—	—	30·1	—	—	—	—
Concl. Ten.,	—	0·188 in.	—	·249	·274	—	—	—	·168	—	—	—	—
Rel. Hum.	—	85%	—	62	58	—	—	—	100	—	—	—	—

(Black-bulb Ther., March 14, 10^h 20^m a.m., 144°·9.)

Vincocaya, 1883, June 23.

	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.	H. M.
App. time,	6 29 a.m.	7 47	8 4	8 16	8 33½	8 56	9 35	1 11 p.m.	2 22	4 35	7 28	7 59	10 41
An. Barom.,	—	17·43	17·44	—	—	—	—	—	—	—	17·44	—	17·51
App. of sky,	*b*3	*b*3 !	*b*3	—	—	*b*4	*b*4	—	*b*3	*b*3	—	—	—
Dry Ther.,	6°·9 ‡	18·9	20·9	21·9	24·0	24·9	29·8	42·9	45·7	41·2	26·6	—	9·1
Wet ,,	—	13·9	15·2	16·1	17·6	18·1	21·6	31·0	30·6	25·8	—	—	5·7
Concl. Ten.,	—	·049 in.	·050	·053	·055	·054	·062	·095	·069	·036	—	—	·066
Rel. Hum.,	—	48%	45	45	43	40	38	34	22	14 §	—	—	52

Excepting the slightest trace of cloud in the N.E. on the peaks of the Alto de Toledo the sky was perfectly clear.

	H. M.		H. M.		H. M.	
Black-bulb Ther.,	8 4 a.m.	107°·5	8 59 a.m.	168°·6	11 26 a.m.	197°·2
	8 10 ,,	130·4	9 30 ,,	175·4	1 53 p.m.	201·5
	8 13 ,,	136·9	10 11 ,,	183·4	2 9 ,,	198·1
	8 24 ,,	150·5	10 52 ,,	194·4	2 22 ,,	199·0
	8 33½ ,,	155·4	11 5 ,,	195·5	4 14 ,,	144·4

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* As already explained in the introduction, the notation "cu ½B" is used to indicate a very small quantity of cloud low down on the lake in the direction of Bolivia.

† These numbers express the blueness of the clear part of the sky, *b* 4 being the most intense dark blue observed.

‡ This is the lowest temperature of the air observed on the journey.

§ These are also the minimal tension and humidity recorded.

The readings of the black-bulb thermometer compared with those at Arequipa and Puno show that the heating effect of the sun's rays is nearly the same at heights of 14360, 12600, and 7500 feet, with a scarcely appreciable increase at the lower stations. The tube of the instrument was, however, too short to permit of its being freely exposed without being watched, as a slight increase of temperature beyond the records actually obtained would probably have burst the bulb. On several occasions the experiments had to be discontinued on this account.

Barometrical Observations.

Although the scale of the small pocket barometer that I had with me is so close that the intervals 17 to 18 ins. and 19 to 20 ins. used at Vincocaya and La Paz are represented respectively by only 0·2 in. and 0·3 in. on the instrument, the daily period was very soon seen to be so strongly marked that I was induced to enter the readings along with those of the thermometers. They have been combined in a slightly more elaborate manner than the temperatures by first obtaining an approximate curve representing the diurnal period and then finding the constants to be applied to each day's readings to bring them to the average height of the curve at the hours of observation. At Puno the changes in pressure from day to day were so small that the largest of these diurnal constants is but 0·14 inch, nor were the corrections appreciably larger at La Paz or Vincocaya although at these stations the movements of the barometer were not so uniform as at Puno. The 579 corrected quantities thus obtained for the last-named station, arranged in exact order of apparent time of the day, were grouped into means of ten each between 8^h a.m. and $0^h\ 28^m$ a.m., the remaining values being combined in the most convenient manner as shown in the table.

Final means of aneroid readings at Puno corrected by a constant value for each date.*
Epoch : 1883, March 20 to June 2.

No. of obs.	App. time. H. M.	Bar. in.	No. of obs.	App. time. H. M.	Bar. in.	No. of obs.	App. time. H. M.	Bar. in.	No. of obs.	App. time. H. M.	Bar. in.
10	0 13 a.m.	18·842	10	10 51 a.m.	18·853	10	2 29 p.m.	18·788	10	6 22 p.m.	18·800
9	0 45	·850	„	10 58	·846	„	2 44	·787	„	6 42	·812
6	1 28	·843	„	11 13	·849	„	3 3	·784	„	7 10	·809
3	2 21	·833	„	11 21	·846	„	3 30	·775	„	7 36	·821
2	3 21	·815	„	11 29	·845	„	3 45	·775	„	8 10	·830
1	4 12	·820	„	11 36	·844	„	4 3	·771	„	8 56	·847
3	5 32	·827	„	11 50	·841	„	4 16	·776	„	9 34	·834
1	6 33	·840	„	12 13 p.m.	·833	„	4 29	·777	„	9 43	·840
4	7 41	·855	„	12 30	·832	„	4 47	·768	„	10 1	·833
10	8 32	·865	„	12 33	·830	„	5 4	·778	„	10 20	·839
„	9 13	·867	„	12 58	·818	„	5 18	·783	„	10 38	·841
„	9 40	·867	„	1 12	·816	„	5 26	·779	„	10 52	·843
„	9 53	·866	„	1 21	·812	„	5 34	·786	„	11 10	·840
„	10 5	·860	„	1 38	·801	„	5 43	·789	„	11 28	·844
„	10 20	·865	„	1 57	·793	„	5 55	·793	„	11 48	·847
„	10 38	·855	„	2 14	·793	„	6 9	·805			

* A few of the readings falling shortly after midnight have been included in the preceding civil day.

From a curve laid down by means of these quantities the following hourly numbers were adopted as representing the daily oscillation of the atmospheric pressure at Puno—

Mean Pressure at Puno.

Hour.	in.	Hour.	in.	Hour.	in.	Hour.	in.
1 a.m.	18·845	7 a.m.	18·846	1 p.m.	18·818	7 p.m.	18·812
2	·837	8	·859	2	·795	8	·827
3	·823	9	·867	3	·781	9	·836
4	·816	10	·865	4	·771	10	·841
5	·821	11	·852	5	·776	11	·846
6	·832	Noon	·837	6	·794	Midnight	·846

The turning points of the curve are—

1st maximum, $12^{h}\ 0^{m}$ p.m. (Midnt.), 18·846 in.; 1st minimum, $3^{h}\ 54^{m}$ a.m., 18·816 in.
2nd maximum, $9^{h}\ 18^{m}$ a.m. .... 18·867 in.; 2nd minimum, $4^{h}\ 16^{m}$ p.m., 18·771 in.

The mean daily range is therefore 0·096 inch.

The absolute maximum pressure observed was 18·91 inches, recorded on four occasions:—April 10, $8^{h}\ 55^{m}$ a.m.; April 10, $12^{h}\ 19^{m}$ p.m.; April 28, $9^{h}\ 51^{m}$ a.m.; and May 28, $11^{h}\ 35^{m}$ a.m. The absolute minimal height was noted on May 26, $4^{h}\ 12^{m}$ p.m. as 18·67 inches. The absolute range observed in 75 days was therefore only 0·24 inch.

In accepting these results it cannot be too clearly insisted on that the reduced heights of the barometer rest solely upon the readings of an imperfectly tested boiling-point thermometer, so that no reliance can be placed on the mean pressure. The diurnal oscillation, on the other hand, is so marked and so thoroughly borne out by the whole of the observations that I cannot but think that the little table just given may be of use in the barometric measurement of heights in the neighbourhood of the Titicaca basin. I hope, too, that it may induce some resident at a considerable altitude, who may be so fortunate as to possess a mercurial barometer, to persevere in making observations at all possible hours instead of confining himself to the customary pair of readings in the twenty-four hours.

At La Paz only 66 readings of the aneroid barometer are available. These reduced and combined in the way already described furnish the subjoined table—

Adjusted Barometric Means. La Paz, 1883, Feb. 18—Feb. 25.

No. of obs.	App. time. H.	App. time. M.	Bar. in.	No. of obs.	App. time. H.	App. time. M.	Bar. in.	No. of obs.	App. time. H.	App. time. M.	Bar. in.	No. of obs.	App. time. H.	App. time. M.	Bar. in.
1	2	31 a.m.	19·400	4	10	1 a.m.	19·408	4	1	42 p.m.	19·368	4	9	58 p.m.	19·405
3	6	46	·418	,,	10	36	·420	,,	2	56	·355	,,	10	54	·419
4	7	44	·425	,,	11	1	·418	,,	4	15	·340	2	11	47	·408
,,	8	13	·422	,,	11	18	·404	,,	7	26	·371				
,,	8	50	·421	,,	12	24 p.m.	·386	,,	8	48	·385				

These quantities are very closely represented by the ordinates of the Puno curve increased by 0·562 inch, and still more closely by also increasing the amplitude of that curve in the proportion of 1·02 to 1·00.

During both visits to Vincocaya the barometric records are not only very incomplete but they also exhibit considerably greater irregularities than at La Paz or Puno. Hence it is hardly practicable to deduce a satisfactory estimate of the extent of the daily oscillation either from the 103 readings made in March or the 61 quantities noted in June. I have therefore combined all together in the following small table—

Adjusted Barometric Means. Vincocaya, 1883, Mar. 1–16, and June 7–26.

No. of obs.	App. time.		Bar. in.	No. of obs.	App. time.		Bar. in.	No. of obs.	App. time.		Bar. in.	No. of obs.	App. time.		Bar. in.
	H.	M.			H.	M.			H.	M.			H.	M.	
7	0	4 a.m.	17·528	12	9	18 a.m.	17·539	12	3	49 p.m.	17·474	12	8	39 p.m.	17·515
4	3	28	·514	12	10	29	·524	12	4	48	·473	12*	9	53	·530
12	6	31	·515	11	11	43	·506	12	6	8	·490	10	10	53	·512
12	7	54	·520	12	1	40 p.m.	·493	12	7	28	·505				

This table does not differ widely from the Puno curve when flattened in the ratio of 0·643 to 1·000 and at the same time lowered by 1·319 inch. This would make the amplitude at Vincocaya 0·034 inch less than at Puno, or would cause a diurnal fluctuation in the difference of height of 47 feet, even when computed from simultaneous readings at both stations. This is a striking indication of the necessity of spreading observations over as much of the twenty-four hours as possible when measuring heights with the barometer.

Conclusion.

It will not have escaped notice that the instrumental outfit at my disposal was not at all adequate for anything deserving the name of an astronomical expedition; nor was it by any means such as I should have elected to take with me had I foreseen that my stay in South America was to last for six months. It was further weakened by the serious damage done to the spectroscope and the equatorial mounting during the long land journey from Tacna. But more than all did I miss the books and star maps which indeed ought never have to have been left behind. Still it is hoped that the foregoing results, however disconnected and imperfect, may be of some use in pointing out the possibilities of a thoroughly planned astronomical expedition to the Titicaca table-lands. From my experience at Puno and the fact that the last rain of the season fell at Vincocaya on March 31, combined with the universally pronounced opinion that the rainy season begins about Christmas it appears that the astronomer can reckon on nearly nine months of good weather or at least weather sufficiently good to furnish abundant astronomical occupation. The reduced pressure of the air of itself suggests an examination of the barometric constant of refraction as promising to be of great interest; and indeed at Vincocaya I felt so curious to test Bessel's assumption that the refraction is proportional to the height of the barometer that I took a series of measures of the nearly vertical distance between δ Argûs and Canopus as the latter star gradually approached the horizon. Unfortunately the little box-sextant employed proved quite insufficient for even the roughtest investigation of the problem, the measures differing occasionally by upwards of 3′. This was the more disappointing as the probable error of a double altitude of the sun was usually below 30″. I attribute this difference in the quality of the observations chiefly to the extremely feeble telescope of the sextant, which failed to show δ Argûs with sufficient distinctness, although it gave sharp images of the sun. However, I had the pleasure of seeing the refraction in distance, which would have been 25′ at the sea-level, reduced to about 15′.*

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* One of the observations has been corrected for an obvious error of 0·05 in.

This great diminution in the refractions while quite in accordance with what was to be expected, leads me to believe that the time will eventually come when an improved transit-circle will be used at a high level in the fundamental branches of practical astronomy, such as the formation of a standard star-catalogue, the determination of the obliquity of the ecliptic and above all in finding the absolute parallaxes of the fixed stars.

This brings us to the important point of the choice of a station, and here it is that I find the best excuse for having dwelt so long on the meteorological materials collected, for, crude as they are, they still show in the clearest manner that the transition from Puno (12600 feet) to Vincocaya, while affording a gain in height or rather the removal of 0·7 inch of atmospheric pressure, has been attended with the sacrifice of 20° F. in a night temperature that is already sufficiently cool at the lower station. The average thermal range and still more the divergence of the extreme temperatures have also been largely increased. Respecting Vincocaya too, I think it is very likely that the dust there thrown up by the winds in the dry season at least fully counteracts any gain there may be in the elevation above Puno. Possibly on further examination it may be found that the summit of one of the numerous islands in the great freshwater lake will afford advantages in the way of greater approach to uniformity of temperature, freedom from dust, and an absence of local disturbance in the air that cannot be found on the mainland. An idea of the climate may be formed from the fact that I saw a fine crop of maize growing within the Inca ruins on the island of Coati. Such a station with the barometer at a nearly uniform height of 18·7 in. and the thermometer between the absolute *extremes* of 70° and 32°, seems almost the ideal site for a permanent observatory. The steamers that ply on the lake would furnish the necessary transport, while native balsas or rush-boats would not be slow in providing most of the ordinary necessaries of life.

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* I take this opportunity of saying a few words of warning about the employment of the box-sextant on land, and especially in the measurement of angles that are not doubled by reflection in an artificial horizon. Delightful an instrument as it is at sea, and for taking double altitudes of the sun on shore, it cannot compare for a moment with the smallest theodolite in the measurement of low altitudes or of horizontal angles, while for depressions it can rarely be used without having recourse to some special expedient.

Respecting other facilities, Arequipa offers those of the large workshops and other resources of the railway, but is at a comparatively low level. La Paz, on the other hand, is at the sufficient height of 12000 feet and as the chief city of Bolivia and also the centre of widely extended commerce offers a residence free from the isolation inseparable from a life on the lake. The immediate neighbourhood however of both cities is too much hemmed in by surrounding mountains or lofty plains to permit of a clear view near the horizon in all directions, while Arequipa, as I have already said, is very liable to earthquake shocks, an evil from which the lake district seems to be in a great measure exempt. The plains between La Paz and the lake offer the choice of all heights between 12500 and about 13300 feet, with a remarkably free horizon and the possibility of communicating with La Paz.

Whatever position might be chosen for a *quasi* permanent observatory, excursions ought certainly to be made to the greatest elevations to which instruments can be carried. From all I could learn, these excursions are best made in the months of September to November when the cold of winter has passed and the rains have not yet set in. At that season Dr. R. Falb and a gentleman with whom I frequently conversed on the subject, passed several days and nights on the snowless summit of the Misti volcano without serious inconvenience, although the height of 18650 feet is necessarily associated with so low a barometer as 14½ ins. I need not here dwell on the extreme interest attaching to measures, at such an elevation, of the radiant heat emitted from the sun, the more so as such determinations could probably be made at all zenith distances down to little short of 92°. Nor would such observations be without their great value at the regular station lower down.

As to the great facilities of reaching this very remarkable or, we may almost say, unique region, they are unrivalled. At the seaport of Mollendo the heaviest cases or castings can be transferred by large launches to the railway-pier where they can be lifted immediately on to the railway-trucks and so carried to any part of the line up to 14666 feet. If it is preferred to go forward into Bolivia the railway train will hand the packages directly over to the steamers on Lake Titicaca and only at the southern end of that inland sea has land carriage by ordinary roads to be resorted to, but even then loads up to two tons in weight can be efficiently dealt with.

In concluding this paper I wish to put on record my extreme indebtedness to the cordial liberality on the part of Lord Crawford which alone rendered my interesting journey possible. Nor can I close this paper without expressing my warmest thanks not only to Mr. J. L. Thorndike, the lessee of the remarkable railroad so often spoken of, and also to its very obliging superintendent Señor Don José M. Braun, but also to each and every member of the staff on that line, with whom I came in contact, as well as to other residents in the district, not only for invaluable assistance, but also for numberless acts of sympathetic kindness of a more personal character.

THE CHROMOSPHERE IN 1883.

By The Rev. S. J. PERRY.

During the past year the Chromosphere has been measured at Stonyhurst around the whole circumference of the Sun on every day entered in the following table, and it has also been partially observed on nine other occasions—

	Number of days of Observation.	Mean height of Chromosphere excluding Prominences.	Mean extent of Prominence arc.	Mean height of Prominences.	Highest Prominence.	Number of days on which lines were observed distorted.
January,	2	8″·5	32° 25′ 30″	34″·2	1′ 12″·2	1
February,	9	8·5	44 11 0	28·0	1 41·6	3
March,	11	8·3	48 3 3	26·5	1 12·6	4
April,	8	8·2	42 37 23	27·2	1 17·8	2
May,	9	8·8	57 17 0	29·7	1 18·6	5
June,	7	8·4	56 28 26	29·8	2 10·3	2
July,	4	8·2	33 46 30	26·3	1 9·2	0
August,	7	8·1	44 42 51	23·4	1 16·0	1
September,	4	7·7	27 39 45	23·2	1 26·7	0
October,	5	8·0	40 12 48	35·8	4 57·7	3
November,	5	8·4	37 16 15	22·4	0 45·3	1
December,	2	8·1	32 10 30	20·3	0 37·2	1
Mean	6·1	8·27	41 24 15	27·23	1 35·43	1·9

An examination of the similar tables of previous years shows a slight increase in all the yearly mean values for 1883, and this is most apparent in the height of the prominences on account of the exceptional prominence observed on Oct. 30th The Chromospheric lines appear to have been distorted on about one-third of the days of observation.

The Prominences exceeded 1′ in height, once in August and in September, twice in January, July, and October, thrice in March, April, and June, and on four days in February and in May. On the 5th of June the extent was above 2′, and on the 30th of October a prominence reached the unprecedented height of almost 5′, or about one-sixth of the solar diameter. This enormous prominence appeared in the shape of a cloud, stretching from 70° 30′ W. of N. to 79° 40′ along the sun's limb, and near the centre of this arc there was a great displacement of the bright C-line towards the red end of the spectrum, and a lesser one towards the violet, at different levels of the solar atmosphere.

On the 16th of May the whole Chromosphere was at an unusually high level, the mean height being 9″·8 ; the prominences however were neither numerous nor very large on that day.

The displacements of the bright C-line, which is invariably used in these measurements, were not so frequent or so exaggerated in 1883 as in the preceding year. There appears to be a decided synchronism between the contortions of the Chromospheric lines and any special developement or rapid alterations of spot areas.

In the great prominence of October 30th, two faint bright lines were distinctly visible on the more refrangible side of C, the separation of the lines being in each case about equal to the distance separating D_1 from D_2. These faint lines were only seen in the more brilliant portions of the prominence, and attained the heights of 59″·8 and 51″·3 above the photosphere. The bright line K. 654·3, between B and C, was observed on several occasions; it was exceedingly bright on May the 30th, at a point of the limb where C was much distorted. Another bright line, K. 534·0, between a and B, was also well observed, but it seems to be much less frequently visible than K. 654·3.

Stonyhurst College Observatory,
January 19th, 1884.

A COMPARISON OF LEVERRIER'S TABLES OF URANUS AND NEPTUNE WITH THOSE BY NEWCOMB OF THE SAME PLANETS.

By DAVID P. TODD, M.A.

I had occasion, several years ago, to institute a detailed comparison of Leverrier's Tables of Uranus and Neptune with those by Newcomb of the same planets. A brief time with the work showed me that there would be very little advantage in completing it according to the original plan, so it was abandoned. A summary of a few of the scattering results in heliocentric longitude is here given.

Heliocentric Longitude of Uranus (Leverrier).

Date. Greenwich Mean Noon.	L	ϖ	θ	f	Perturbations by			Tables		ρ	v
					Jupiter.	Saturn.	Neptune.	XIV.	XXV.		
1864, Jan. 1,	88°36′ 47″·14	168°29′27″·8	73° 18′ 7″·3	– 5°20′35″·15	+ 34″·82	– 96″·11	– 62″·55	+ 1″·72	+ 0″·13	– 3″·18	83° 14′ 6″·82
1868, Jan. 1,	105 48 31·87	168 33 14·5	73 19 19·6	– 4 54 33·44	– 48·30	– 110·97	– 49·95	+ 0·59	+ 0·03	– 7·66	100 50 22·17
1873, Aug. 1,	129 48 27·52	168 38 31·6	73 21 0·5	– 3 32 3·95	+ 29·75	– 123·64	– 26·68	– 1·55	– 0·18	– 9·00	126 14 12·27
1888, Jan. 1,	191 47 17·28	168 52 13·0	73 25 21·1	+ 2 13 1·34	+ 34·54	– 47·37	+ 28·94	– 1·74	– 0·88	+ 8·19	194 0 40·30
1892, Jan. 1,	208 59 2·72	168 56 1·8	73 26 33·6	+ 3 37 40·38	+ 34·33	– 3·08	+ 34·77	– 0·99	– 1·04	+ 9·26	212 37 56·35
1896, Jan. 1,	226 10 48·27	168 59 51·5	73 27 45·9	+ 4 40 23·79	– 50·31	+ 32·53	+ 35·11	– 1·29	– 1·17	+ 6·64	230 51 33·57
1900, Jan. 1,	243 22 33·91	169 3 40·4	73 28 58·3	+ 5 16 0·17	– 11·28	+ 58·36	+ 30·77	– 1·53	– 1·25	+ 1·56	248 39 50·71
1904, Jan. 1,	260 33 37·33	169 7 30·1	73 30 10·8	+ 5 22 29·59	+ 52·28	+ 83·79	+ 23·28	– 2·42	– 1·29	– 3·94	265 58 38·62
1912, Jan. 1,	294 57 9·22	169 15 10·5	73 32 35·6	+ 4 13 54·20	– 44·68	+ 111·28	+ 5·36	– 3·60	– 1·25	– 9·36	299 12 1·17
1920, Jan. 1,	329 20 41·56	169 22 52·2	73 35 0·6	+ 1 45 6·84	+ 17·93	+ 75·24	– 7·18	– 2·80	– 1·08	– 3·94	331 7 6·57
1940, Jan. 1,	55 19 34·42	169 42 12·4	73 41 3·0	– 4 47 58·48	– 32·13	– 69·74	+ 18·59	– 0·75	– 0·76	+ 6·78	50 30 17·93

$$v = \text{Heliocentric longitude (mean Equinox)} = \mathrm{L} + f + \text{perturbations by} \left\{ \begin{matrix} \text{Jupiter} \\ \text{Saturn} \\ \text{Neptune} \end{matrix} \right\} + \text{Tables} \left\{ \begin{matrix} \text{XIV.} \\ \text{XXV.} \end{matrix} \right\} + \rho$$

The position of Uranus for the 1st August, 1873, is computed as the example in the precepts for the use of the Tables by Leverrier.

Heliocentric Longitude of Uranus (Newcomb).

Date. Greenwich Mean Noon.	g	ω	θ	E	Perturbations.	R	v
1864, Jan. 1,	280° 9′ 52″·56	95° 5′ 22″·31	73° 18′ 18″·06	− 5° 20′ 24″·85	+ 51″·36	+ 6″·83	83° 14′ 6″·27
1868, Jan. 1,	297 18 15·98	95 7 29·03	73 19 32·39	− 4 54 14·01	− 45·15	+ 2·32	100 50 20·56
1888, Jan. 1,	23 0 14·70	95 18 2·44	73 25 44·21	+ 2 13 13·45	+ 180·74	+ 18·21	194 0 33·75
1892, Jan. 1,	40 8 38·78	95 20 9·10	73 26 58·62	+ 3 37 35·82	+ 247·25	+ 19·27	212 37 48·84
1896, Jan. 1,	57 17 2·96	95 22 15·74	73 28 13·03	+ 4 40 0·37	+ 217·02	+ 16·65	230 51 25·77
1900, Jan. 1,	74 25 27·25	95 24 22·37	73 29 27·46	+ 5 15 18·06	+ 297·09	+ 11·57	248 39 43·80
1904, Jan. 1,	91 33 9·41	95 26 28·92	73 30 41·86	+ 5 21 31·40	+ 395·01	+ 6·05	265 58 32·65
1912, Jan. 1,	125 49 58·53	95 30 42·14	73 33 10·79	+ 4 12 39·05	+ 326·22	+ 0·64	299 11 57·37
1920, Jan. 1,	160 6 48·10	95 34 55·32	73 35 39·77	+ 1 43 58·80	+ 335·42	+ 6·05	331 7 3·46
1940, Jan. 1,	245 48 53·91	95 45 28·11	73 41 52·45	− 4 47 3·62	+ 44·46	+ 16·78	50 30 12·09

v = Heliocentric longitude (mean Equinox) $= g + \omega + \theta + \mathrm{E} + \text{perturbations} + \mathrm{R}$

Newcomb's Tables of Uranus were used in the preparation of the British Nautical Almanacs for 1877, 1878, 1879, 1880, and 1881; while Leverrier's Tables have been employed in the six subsequent issues of that publication. The longitude of Uranus for the beginning of each of these years is included in the following table, and makes the comparison more complete.

All the data for the comparison of the longitudes of Uranus from the two sets of Tables are here collated :—

Comparison of Longitudes of Uranus.

Date. Greenwich Mean Noon.	Hel. Long. from British Naut. Alm. (True Equinox).	Nutation.	Heliocentric Longitude (mean Equinox).		Correction to Newcomb.
			Leverrier.	Newcomb.	
1864, Jan. 1,	. . . . .	. . .	83° 14′ 6″·82	83° 14′ 6″·27	+ 0″·55
1868, Jan. 1,	. . . . .	. . .	100 50 22·17	100 50 20·56	+ 1·61
1873, Aug. 1,	. . . . .	. . .	126 14 12·27	126 14 9·65	+ 2·62
1882, Jan. 1,	165° 41′ 15″·8	− 16″·53	165 40 59·27	165 40 54·29	+ 4·98
1883, Jan. 1,	170 24 45·0	− 13·48	170 24 31·52	170 24 26·14	+ 5·38
1884, Jan. 1,	175 8 20·9	− 8·88	175 8 12·02	175 8 6·49	+ 5·53
1885, Jan. 1,	179 52 38·0	− 3·31	179 52 34·69	179 52 29·30	+ 5·39
1886, Jan. 1,	184 35 50·2	+ 2·62	184 35 52·82	184 35 47·14	+ 5·68
1887, Jan. 1,	189 18 31·2	+ 8·20	189 18 39·40	189 18 33·20	+ 6·20
1888, Jan. 1,	. . . . .	. . .	194 0 40·30	194 0 33·75	+ 6·55
1892, Jan. 1,	. . . . .	. . .	212 37 56·35	212 37 48·84	+ 7·51
1896, Jan. 1,	. . . . .	. . .	230 51 33·57	230 51 25·77	+ 7·80
1900, Jan. 1,	. . . . .	. . .	248 39 50·71	248 39 43·80	+ 6·91
1904, Jan. 1,	. . . . .	. . .	265 58 38·62	265 58 32·65	+ 5·97
1912, Jan. 1,	. . . . .	. . .	299 12 1·17	299 11 57·37	+ 3·80
1920, Jan. 1,	. . . . .	. . .	331 7 6·57	331 7 3·46	+ 3·11
1940, Jan. 1,	. . . . .	. . .	50 30 17·93	50 30 12·09	+ 5·84

The longitudes from Newcomb's Tables for January 1 of 1882–3–4–5–6–7 (opposite those from Leverrier extracted from the British Nautical Almanac) are taken directly from the American Ephemeris. The position for 1873, August 1, from Newcomb's Tables is interpolated back from the subjoined series of co-ordinates derived by Mr. William Ferrel, who formerly computed Uranus for the American Ephemeris. By permission of Professor Newcomb, the Superintendent of that publication, these heliocentric co-ordinates, not given in the American Ephemeris previously to the issue for the year 1882, are here presented, thus giving, with the subsequent issues of the Ephemeris, a continuous series of heliocentric co-ordinates of Uranus, from the epoch of publication of the tables onward.

Heliocentric co-ordinates of Uranus (Newcomb).

Date. Greenwich Mean Noon.	Longitude (Mean Equinox).	Latitude.	Log *r*.
1873, August 27,	126° 33′ 54″·69	+ 0° 37′ 7″·34	1·2672032
December 25,	128 5 8·29	0 37 50·97	1·2668454
1874, April 24,	129 36 29·91	0 38 32·95	1·2664968
August 22,	131 7 59·10	0 39 13·26	1·2661574
December 20,	132 39 35·56	+ 0 39 51·94	1·2658276
1875, April 19,	134 11 18·92	0 40 28·96	1·2655076
August 17,	135 43 8·83	0 41 4·27	1·2651981
December 15,	137 15 5·01	0 41 37·86	1·2648998
1876, April 13,	138 47 7·06	+ 0 42 9·70	1·2646133
August 11,	140 19 14·74	0 42 39·73	1·2643391
December 9,	141 51 27·71	0 43 7·91	1·2640774
1877, April 8,	143 23 45·73	0 43 34·24	1·2638293
August 6,	144 56 8·56	+ 0 43 58·69	1·2635954
December 4,	146 28 35·92	0 44 21·30	1·2633761
1878, April 3,	148 1 7·51	0 44 42·04	1·2631720
August 1,	149 33 43·15	0 45 0·82	1·2629836
November 29,	151 6 22·76	+ 0 45 17·65	1·2628112
1879, March 29,	152 39 6·16	0 45 32·51	1·2626550
July 27,	154 11 53·18	0 45 45·40	1·2625153
November 24,	155 44 43·73	0 45 56·29	1·2623924
1880, March 23,	157 17 37·58	+ 0 46 5·19	1·2622862
July 21,	158 50 34·65	0 46 12·14	1·2621966
November 18,	160 23 34·79	0 46 17·11	1·2621239
1881, March 18,	161 56 37·83	0 46 20·05	1·2620673
July 16,	163 29 43·53	+ 0 46 20·99	1·2620265
November 13,	165 2 51·68	0 46 19·93	1·2620013
1882, March 13,	166 36 2·24	0 46 16·83	1·2619923
July 11,	168 9 14·62	0 46 11·69	1·2619982
November 8,	169 42 28·45	+ 0 46 4·55	1·2620187

At the time of publication of Leverrier's Tables of Uranus, the planet was farther from his positions than from Newcomb's, and recent observations show that Uranus is now about midway between the two; the correction to Leverrier's longitudes being minus, and to Newcomb's plus. During the last decade of the present century, however, Leverrier's Tables appear likely to be somewhat more accurate than Newcomb's.

Heliocentric Longitude of Neptune (Leverrier).

Date. Greenwich Mean Noon.	L	f	Perturbations by			ρ	v
			Jupiter.	Saturn.	Uranus.		
1873, Aug. 1,	26° 57′ 0″·79	− 16′ 59″·19	+ 26″·14	− 19″·19	+ 7″·12	− 22″·94	26° 39′ 52″·73
1886, Jan. 1,	54 15 19·23	+ 10 47·17	+ 30·81	+ 11·58	+ 5·26	+ 23·31	54 27 17·36
1890, Jan. 1,	63 3 0·02	+ 19 31·24	− 24·50	+ 18·17	+ 3·60	+ 35·61	63 23 4·14
1894, Jan. 1,	71 50 40·76	+ 27 47·43	− 6·90	+ 15·44	+ 1·63	+ 44·50	72 19 22·86
1898, Jan. 1,	80 38 21·43	+ 35 23·91	+ 35·39	+ 5·65	− 0·66	+ 49·15	81 15 14·87
1910, Jan. 1,	107 1 1·37	+ 52 33·02	+ 34·31	− 18·18	− 7·30	+ 35·58	107 54 18·80
1930, Jan. 1,	150 59 22·78	+ 56 3·50	− 32·29	+ 16·90	− 11·20	− 33·09	151 54 26·60

$$v = \text{Heliocentric longitude (mean Equinox)} = \mathrm{L} + f + \text{perturbations by} \left\{ \begin{array}{l} \text{Jupiter} \\ \text{Saturn} \\ \text{Uranus} \end{array} \right\} + \rho$$

The position of Neptune for the 1st August, 1873, is computed as the example in the precepts for the use of the Tables by Leverrier.

Heliocentric Longitude of Neptune (Newcomb.)

Date. Greenwich Mean Noon.	l	y	θ	E	$\Sigma dv_{1 \ldots 9}$	$\Sigma \left\{ \begin{array}{c} \text{terms in} \\ \text{sin \& cos} \\ \text{of } l \,\&\, 2l \end{array} \right\}$	R	v
1873, Aug, 1,	° ′ ″	° ′ ″	° ′ ″	° ′ ″	″	″	″	26° 39′ 51″·49
1886, Jan. 1,	53 44 54·98	228 58 45·40	130 30 23·97	1 9 25·43	119·46	+ 20·82	83·25	54 27 13·31
1890, Jan. 1,	62 29 13·99	228 59 27·36	130 33 3·05	1 18 11·16	70·10	+ 17·05	95·57	63 22 58·28
1894, Jan. 1,	71 13 32·92	229 0 9·30	130 35 42·16	1 26 29·29	84·02	+ 12·90	104·49	72 19 15·08
1898, Jan. 1,	79 57 51·77	229 0 51·24	130 38 21·27	1 34 7·87	115·45	+ 8·58	109·14	81 15 5·32
1910, Jan. 1,	106 10 26·33	229 2 56·96	130 46 18·60	1 51 22·88	84·45	− 3·31	95·62	107 54 1·53
1930, Jan. 1,	149 51 58·28	229 6 26·33	130 59 34·59	1 54 56·90	46·20	− 12·76	26·96	151 53 56·50

$$v = \text{Heliocentric longitude (mean Equinox)} = l + y + \theta + \mathrm{E} + \Sigma dv_{1 \ldots 9} + \Sigma \left\{ \begin{array}{c} \text{terms in} \\ \text{sin and cos} \\ \text{of } l \text{ and } 2l \end{array} \right\} + \mathrm{R}$$

Newcomb's Tables of Neptune were used in the preparation of the British Nautical Almanacs, 1871–1881 inclusive, and the position of the planet for 1873, August 1, is derived from that source.

In the following table, the longitudes from Newcomb's Tables for January 1 of 1882–3–4–5–7 (opposite those from Leverrier extracted from the British Nautical Almanac) are taken from the American Ephemeris. For 1886, Jan. 1, I have given the preference to my own computation from both Tables, as the hundredths of a second were carefully preserved, while in both the Almanacs the positions are given to tenths of a second. Had the Almanac-positions been used, the "Correction to Newcomb," for this date, would have been + 3″·98, instead of + 4″·05 as given below.

It should be remarked here that the heliocentric longitudes of Neptune from Newcomb's Tables published in the American Ephemeris for 1885 and 1886, need a small systematic correction, as follows:—

1885, Mar. 27,	0″·00
1886, Jan. 3,	− 0·80
Dec. 29,	− 1·78

The correction varies quite uniformly with the time, and has been appropriately applied in the following table.

All the data for the comparison of the longitudes of Neptune from the two sets of Tables are here collated—

Comparison of Longitudes of Neptune.

Date. Greenwich Mean Noon.	Hel. Long. from British Naut. Alm. (True Equinox).	Nutation.	Heliocentric Longitude (Mean Equinox).		Correction to Newcomb.
			Leverrier.	Newcomb.	
1873, Aug. 1,	[26° 39′ 39″·7	+ 11″·79]	26° 39′ 52″·73	26° 39′ 51″·49	+ 1″·24
1882, Jan. 1,	45 30 2·5	− 16·53	45 29 45·97	45 29 43·01	+ 2·96
1883, Jan. 1,	47 44 27·4	− 13·48	47 44 13·92	47 44 10·90	+ 3·02
1884, Jan. 1,	49 58 44·9	− 8·88	49 58 36·02	49 58 32·84	+ 3·18
1885, Jan. 1,	52 13 15·4	− 3·31	52 13 12·09	52 13 8·55	+ 3·54
1886, Jan. 1,	54 27 14·7	+ 2·62	[54 27 17·36	54 27 13·31]	+ 4·05
1887, Jan. 1,	56 41 6·5	+ 8·20	56 41 14·70	56 41 10·24	+ 4·46
1890, Jan. 1,	. . . .	. . .	63 23 4·14	63 22 58·28	+ 5·86
1894, Jan. 1,	. . . .	. . .	72 19 22·86	72 19 15·08	+ 7·78
1898, Jan. 1,	. . . .	. . .	81 15 14·87	81 15 5·32	+ 9·55
1910, Jan. 1,	. . . .	. . .	107 54 18·80	107 54 1·53	+ 17·27
1930, Jan. 1,	. . . .	. . .	151 54 26·60	151 53 56·50	+ 30·10

Leverrier's Tables of Neptune being founded on nine years more observations than Newcomb's, the planet is now following the former tables much more accurately than the latter. The present correction to Leverrier's heliocentric longitudes is about − 0″·5, while the planet is about 2″·5 in advance of Newcomb's positions. It seems likely that, by the end of the present century, Newcomb's tables will be largely in error, probably in the neighbourhood of 10″.

Lawrence Observatory,
Amhurst, Mass., U.S.A.,
20th December, 1883.

THE LATE PROFESSOR KLINKERFUES.

Ernst Friedrich Wilhelm Klinkerfues was the eldest son of the surgeon of a Hessian regiment of Hussars, and was born at Hofgeismar on March 29, 1827. When four years old, on the occasion of his father's transference to the hospital at Spangenberg, he was left behind at Hofgeismar in the care of two childless maternal aunts, whose special favourite he had become. These good ladies seem to have left their little charge very much to follow the bent of his own inclinations which led him at a very early age to find in books almost his only pleasure and occupation. From the ordinary burgher school he was removed to a private school in his tenth year, and four years later to the Gymnasium at Cassel, where he found a home in the house of his uncle, the late Obergerichtsrath Dedolph. Here too he seems to have been a shy, studious boy, taking little part in the ordinary amusements of his schoolfellows, although his affectionate disposition led him all the more keenly to enjoy the vacations spent with his brothers and sisters under their father's roof at Spangenberg. Originally it was intended that he should study theology, but his early interest in the natural sciences caused this plan to be abandoned while he was yet in the *secunda* of the gymnasium, which he relinquished for the Polytechnic School, carrying with him the most flattering credentials from Dr. Weber, then director of the gymnasium. On the death of his uncle Dedolph, Klinkerfues took lodgings in the house of the optician Fiorino, in Cassel, and thus fell in with some old astronomical instruments, one of them being a reflecting telescope. He at once conceived a most ardent desire to learn the use of these instruments, a desire the more easily gratified in part, as his room in the "Colonnades" had a kind of balcony affording a free prospect of the heavens. At this time he began to study with the greatest zeal various astronomical books procured from the public library in Cassel, the Hessian *Landes-bibliothek.* Thanks to his training at the gymnasium, he was able to study Lalande's *Astronomie*, and later on Gauss' *Theoria Motus* in the originals. After distinguishing himself at the Polytechnic School for industry and good behaviour, Klinkerfues passed his examination as surveyor (*Geometer*) in 1844 and eventually in 1846 was employed in the construction of the Main-Weser Railway, but this did not prevent him from still drawing largely on the Cassel library for a supply of his favourite literature. From his studious habits Klinkerfues was at this time familiarly known to his friends by the by-name of "the Candidate," a name that he heard not unwillingly in after years.

The then director of the works on the Main-Weser Railway was Obergerichtsrath Weiffenbach, a warm friend of the young engineer's family, who was not slow in recognising the talents of Klinkerfues and moving him to the more important station of Marburg, at the same time expressly according in the document confirming the transference the privilege of attending any of the courses of lectures at that university, and authorizing the necessary exemption from the regular office hours. In this way Klinkerfues retained his post on the railway and heard the lectures of the late Professor Gerling, besides taking part in the practical work at the Observatory, and in common with a large class of zealous students, amongst whom were Professors Tyndall and Schönfeld, in the registration of the deflections of the magnetic needle. Klinkerfues' earliest observations, of an eclipse of the moon, some occultations and also of Ceres and Neptune, are published in the 30th volume of the *Astronomische Nachrichten*. Late in the spring of 1851 Klinkerfues resigned his post and went to Göttingen with the warmest recommendations of Gerling and with the somewhat reticent approval of Gauss, as we have just learnt through the publication, in number 2573 of the journal just mentioned, of a most interesting letter from the great mathematician to Gerling, dated May 10, 1851. It is pleasant however to find that the same letter expresses the generous apprehension that undue caution on the writer's part may possibly have shaken Klinkerfues' intention of coming to Göttingen. In that University however Klinkerfues took up his residence in the summer session of 1851, and so far satisfied Gauss of his capabilities that he was installed assistant at the observatory at Michaelmas in the same year, having already taken part with Gauss and J. G. Westphal in the observation of the eclipse of the sun on July 28. That autumn he computed two sets of elements of Eunomia, the first along with Westphal, who held the post of "Observator;" these were followed by elements of Comet IV., 1851. Shortly afterwards Klinkerfues computed the difficult orbit of Psyche, preparing the way to the rediscovery of that planet in February, 1853. It is well known that Gauss was much gratified with the result of this application of a special method given in the *Theoria Motus*. In June, 1853, Klinkerfues became more widely known through the discovery of Comet III. of that year which afterwards became so bright as to be observed in the daytime at Olmütz and at Liverpool. This was the first of Klinkerfues' discoveries in this field, to which he afterwards added five other comets—1854 III., 1854 IV., 1857 III., 1857 V. and 1863 II.—besides independently finding Comets 1854 I. and 1855 II., the latter only one day after its discovery by Donati. To return to the year 1853—to all outward appearance nothing could be more promising than the career of the young astronomer, actively engaged in advanced theoretical studies, favourably known as a discoverer and possessed of a phenomenal facility in the performance of elaborate calculations, and yet even at this early stage the infinitesimal smallness of the remuneration allotted to him had brought about a state of indebtedness from which there is reason to fear he never had the energy to escape.

Gauss died on February 23, 1855, and the Göttingen Observatory was placed under the nominal direction of the well-known Professor W. Weber, with Klinkerfues as "Observator" in the place of Westphal, resigned. In this year Klinkerfues took his degree of Doctor of Philosophy, his inaugural dissertation being the essay

Ueber eine neue Methode die Bahnen der Doppelsterne zu berechnen. In this year also he was made an Assessor of the Royal Society of Sciences of Göttingen, but was unfortunately destined never to become a full member of that body. In the modest post of "observator" he continued his investigations respecting Psyche and other minor planets besides zealously and successfully searching for comets as already mentioned, and further extending and applying his theory of the motions of double stars. About this time too an extensive series of zone-observations were commenced in conjunction with Dr. afterwards Professor Schering and various successive assistants. The method of recording the declinations in these zones much resembled that introduced with such signal results by Gauss for following the variations of the magnetic needle. A mirror was fixed to the transit-circle and in it were observed the reflected divisions of a fixed ivory scale as seen through a large transit instrument mounted in the same meridian as the transit-circle a few paces to the north. Much labour was bestowed upon these observations and about 6000 mean places for 1860·0 were deduced, but owing it is believed to the discovery of certain systematic errors they have not yet been published; there are besides, according to Professor Schering about as many unreduced observations, and one cannot but join in his wish that these may eventually be utilized (see A. N. No. 2573).

Klinkerfues was appointed Provisional Director of the Observatory in 1859, Extraordinary Professor of Astronomy in 1863, and eventually the Observatory was in 1868 divided into two departments, one of practical astronomy, of which Klinkerfues was appointed Director, and one of geodesy, which was put under the charge of Professor Schering.

During these years of tardy advancement Klinkerfues never managed to shake off the load of debt that oppressed him, and only to his singularly elastic and genial temperament can it be ascribed that he was still able to abstract himself from his troubles and labour with success in the enchanting field of original investigation. About 1864 he became absorbed in optical studies, more particularly in that of the refrangibility of light as affected by the motion of its source. In these difficult investigations his own experiments with an achromatic prism in 1865 led to no satisfactory results, although they were not without their value in directing attention to the subject afterwards successfully demonstrated with the dispersive prism in the hands of Dr. Huggins and others. In like manner his imperfect experiments on the possible dependence of the constant of aberration on the thickness of the object-glass of the telescope employed served but to prepare the way to the complete settlement of the question by Sir George Airy. About this time Klinkerfues published a translation (into German) of Briot's *Essai sur la théorie mathématique de la lumière*, and an original work entitled *Die Aberration der Fixsterne nach der Wellentheorie.*" To these may be added a translation with additions of Dr. Huggins' Nottingham lecture on Spectrum Analysis as applied to the heavenly bodies, which has passed through more than one edition.

In his character of professor, Klinkerfues usually read three courses of lectures—on spherical, on theoretical and on perturbational astronomy. In each of these departments his teaching was distinguished by the great variety of his methods of exposition. In his hands the most hackneyed subject assumed some fresh interest or was regarded from some new and unexpected point of view. This is clearly exemplified in his published work, "*Theoretische Astronomie*," which will long remain a favourite work amongst students of astronomy. In this work the relation between shooting stars and comets is dwelt on at length in a special section in which the term "radiant of convergence" is introduced, hence on the appearance of the great display of shooting stars on November 27, 1872, Klinkerfues quickly came to the conclusion that the departing swarm of meteors ought to be visible in the neighbourhood of θ Centauri. How his telegram to Madras led to the observation by Mr. Pogson on two occasions of a cometic body in the region of the heavens indicated, is a part of the history of astronomy, although it is not universally admitted that the object so seen was associated either with the meteoric display or with Biela's comet. Should the coincidence, after all, have been only accidental it would indeed be an occasion for the application of Schiller's lines already quoted by Sir John Herschel in another connexion:—

> "Mit dem Genius steht die Natur in ewigem Bunde;
> Was der Eine verspricht, leistet die Andre gewiss."

Later on he devoted much care and thought to the improvement of the hair hygrometer and eventually produced a bifilar instrument of which the theory is as elegant as it is interesting. This instrument when tested at even wide intervals is calculated to be of great use whenever it is desirable to know the relative humidity of the air at a glance. An attempt to utilize this instrument in the local prediction of weather, notably of rainfall, was not without a considerable measure of success, although from the very nature of the problem its applicability to large areas may well be doubted. The invention of a hydrostatic gas-lighter and various forms of range-finder might here have been passed over in silence, but that they led him into business engagements which he was by nature totally unfitted to manage and which but added to the inextricable confusion of his monetary affairs. One thing however is certain, that in all the endeavours to turn these really ingenious inventions to account, the end aimed at was to procure not only a sufficient sum to discharge his obligations, but also to purchase for his observatory a suitable instrument to occupy the still vacant dome that had originally been constructed with the approval of Jerome Bonaparte when King of Westphalia.

Professor Klinkerfues' contributions to the daily journals ranged over a great variety of subjects, but all his papers were singularly readable and often contained the most original and suggestive ideas. Occasionally indeed they were altogether of a quaintly humerous character, introducing, for instance, the alleged wonderful discoveries of an imaginary Professor Monkhouse, who read the past history of our globe in the reflections from the surfaces of the fixed stars. The Royal Society's Catalogue enumerates 52 of his scientific papers in addition to 9 published in conjunction with J. G. Westphal. More than thirty years ago he was nominated corresponding member of the Academy of Antwerp and was subseqently decorated with the order of the Guelph by the late King of Hanover, at whose instance he observed the total eclipse of the sun on 18 July, 1860, at Cullera, in Spain.

It would be useless and almost impossible to attempt to describe how the warm-hearted and genial astronomer failed to take that position amongst his colleagues to which his undoubtedly great natural talents entitled him. His extreme carelessness of late years in his outward appearance was certainly much against him, but the unflagging zeal with which he delivered a whole course of lectures, if need were, even to a single student, ought, and doubtless in some cases did tell in his favour. But on one ground or other the title of *Professor ordinarius* was withheld from him to the last, although eventually the equivalent stipend was conferred upon him. Still the pleasure the official recognition of his ability would have afforded him may be gathered from the just pride and evident satisfaction with which he regarded his election as Associate of the Royal Astronomical Society. But nothing could dispel the gloom that settled over him with the complete wreck of all his financial plans to which was added the load of failing health, perhaps mental as well as bodily, until at last he voluntarily sought in death a release from that strained life to which no further ties bound him. This took place on January 28th, 1884. He was followed to the grave by one of his surviving brothers, by the Prorector of the University, the Mayor of Göttingen and also by a large number of students and citizens, but no officiating clergyman appeared to conduct the usual service. The dilemma was solved by a silent prayer and the deposition of the wreath-covered coffin in its last resting place amidst the solemn sounds of a choral psalm.

RALPH COPELAND.

RECENT LITERATURE.

Report on the Solar Eclipse of July, 1878. *By Cleveland Abbe* (Professional Papers of the Signal Service No. 1, Washington, 1881, 186 pp. and 28 plates, 4to; also printed in the Annual Report of Chief Signal Officer for 1880, 8vo). This volume contains eighty short reports from observers at the stations of the U.S. Signal Corps (mostly non-commissioned officers and privates) on the duration of the eclipse, appearance of the Corona &c., and a special report on the expedition under the late General Myer to Pike's Peak, Colorado (14,147 feet over Sea level). The sketches of the Corona differ as usual a good deal from each other, but the main features can be recognised on almost all of them.

In the notice of the *Logarithmisch-trigonometrisches Handbuch auf fünf Decimalen*, on p. 52 of this volume, are two errors; in line 6 in place of "average differences" ought to have been "to the right are given the differences of the last logarithm in each line from the first logarithm in the succeeding line and tables, &c." In line 11, the words "here too the mean differences are given, but without auxiliary tables" are to be struck out. The clause should stand "on the outer edge are given the valid decimals of the log. secant, respectively log. cosecant for each minute, thus rendering this table practically complete for all the usual functions."

Sur quelques anomalies apparentes dans les queues cométaires. Par Th. Bredichin (Astr. Nachr. No. 2568). As our space did not permit the insertion of a paper sent us on this subject, the Author has requested us to call attention to this paper, in which he maintains the correctness of his views as regards the comet of 1744, notwithstanding the objections raised by M. Schwedoff.

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The Publication of this Journal is herewith discontinued.

STORAGE